MOUNDS OF THE NEAR EAST

¶ *Mounds of the Near East* surveys the course and development of archaeological research in Iraq, Syria and Anatolia during its most formative years, in particular since the end of the 1914-18 war. From 1929 until 1961 the author played a central role in that development. This gives the book added importance, as a primary source. Furthermore, Professor Seton Lloyd sets out to explain and to defend the methods adopted by archaeologists in the Near East to resolve the peculiar problems of excavating mud-brick mounds, and to show that this is a specialised form of archaeology, which requires specialised training. This makes for stimulating reading, particularly for those trained in the belief that the techniques applicable to classical archaeology in Europe have universal validity.

¶ The author is Professor of Near Eastern Archaeology in the University of London, and, from 1946 to 1961, was Director of the British Institute of Archaeology, Ankara.

¶ The drawing on the cover is of the largest of the 'worshipper' statues discovered in the excavation of Tell Asmar.

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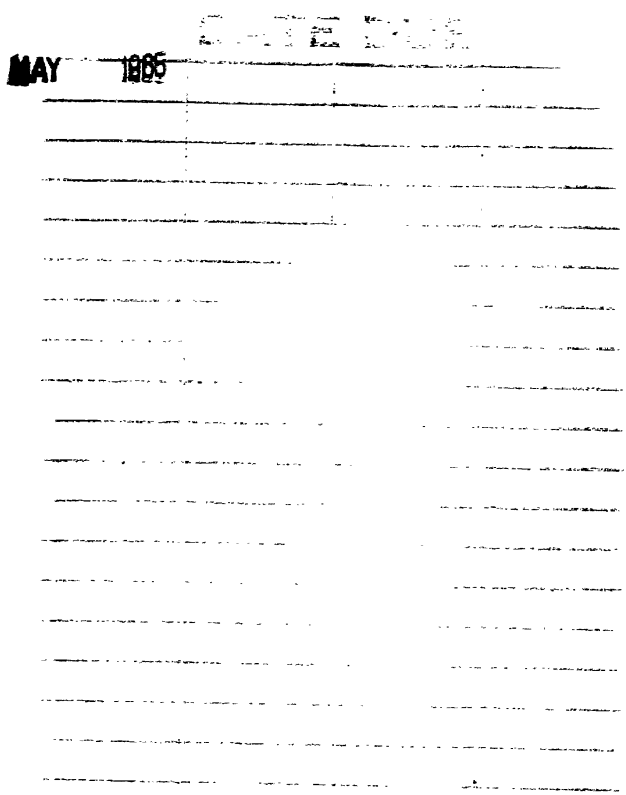

MOUNDS OF THE NEAR EAST

SETON LLOYD

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PREFACE

If a work of this calibre merits the dignity of a dedication, it is to my fellow-workers of several nationalities in the field of Near Eastern archaeology that it should be directed, in affection and intermittent nostalgia. Their names appear in these pages with a frequency which is in proportion to my admiration for their ability.

The substance of the book was given in the Rhind Lectures for 1962, and I am grateful to the University of Edinburgh for the opportunity to articulate the views which it contains. In particular, I am indebted to two Edinburgh scholars, Professors D. Talbot Rice and Stuart Piggott for their encouragement, while exonerating them from any complicity in the expression of my opinions. Among those to whom my thanks are due for permission to use illustrations are The Oriental Institute of the University of Chicago, The Iraq Government Directorate General of Antiquities, the British Institute of Archaeology at Ankara and the Clarendon Press.

University of London,
Institute of Archaeology, 1963.

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INTRODUCTION

In introducing this book, there are good reasons why one should start with a reassurance to the reader. In our own time, remarkable advances have been made in the improvement of archaeological method and practice: and during the past decade, a dozen handbooks have been produced, explaining the principles involved, illustrating their effectiveness and advocating their adoption. One would think there is hardly room for another: and indeed, this is not intended to be one. It will deal with a particular aspect of archaeology; a particular kind of archaeological excavation, as practised in one very vitally important group of countries. Even so, it will not pretend to be a comprehensive survey of what has been done there or of the methods adopted. For it is based largely on the personal experience of a particular excavator. Its primary purpose is to share that experience and the knowledge derived from it, with others who may benefit from it, before the pattern of recollected inferences loses its precision.

When, in 1961, I gave up my appointment as director of the archaeological institute in Ankara, I had almost completed a third-of-a-century of continuous residence in the Near East: and during that period, hardly a year passed when I was not engaged in the supervision of an archaeological excavation. Almost all these excavations took place within the limits of what the American historian, James Henry Breasted, first called the "Fertile Crescent": and the use of that phrase introduces the necessity for an immediate definition of the geographical area which the book is intended to cover. And here it at once becomes apparent that Breasted's phrase will not serve our purpose. It was intended as an imaginative delimitation of the Near Eastern area, in which human beings, living for the first time in settled communities, developed the earliest symptoms of civilization. But it has more recently been distorted by political use, (sometimes to exclude Egypt), and even within the scope of its original intention, progressive archaeological research has modified its shape, until the simulation of a crescent must tax the ingenuity of a cartographer. And in any case, rather than the homeland of the first farmers, our present intention is to define the area in which the remains of their settlements take a particular and distinctive form; in which, whether they have continued to be mere villages or been promoted by historic circumstances to the status of cities, they have created tumuli of occupational debris of the form and composition to which the name "mound" is usually given. The countries to which

this applies include, within the original "crescent", Egypt, Palestine, Syria, Iraq and part of Iran. Beyond these it applies to the whole of Anatolia, to Thessaly on the European mainland and to a wide area in the interior of the Balkan Peninsula. Eastwards, the same peculiar phenomenon also extends through Afghanistan to the Indus Valley. But it is not with these remoter countries that I intend to deal. In the pages which follow, if the validity of personal experience is to be preserved, only Anatolia may be added to the countries of the original "crescent".

These mounds, then, of the Near East, represent the superimposed remains of human settlements, repeatedly destroyed and rebuilt over periods of time which may in some cases amount to as much as nine thousand years. Their actual anatomy is of course extremely complicated and clearly it may vary very considerably from country to country and place to place, as a result of variations in climate and also in building materials. But they have certain primary factors in common; and these collective peculiarities are intended to form the central theme of the present work. For an exposition of their character will, it is hoped, provide material for an argument in support of a personal contention, upon whose vindication the success of the book depends. It is the similarities rather than the diversities of ancient mounds which have captured one's attention during long experience of their excavation. In the end, the conviction has grown upon one that, whether the site be in Palestine or Afghanistan—whether the mound ceased to be occupied in three thousand B.C. or whether it is still covered by the dwellings of a modern village, the archaeologist who undertakes its excavation will find himself faced by the same cardinal problems. Both his technical procedure and the pattern of his subsequent deductive reasoning will be governed by a constant and unvarying code of rules, applicable only to this form of excavation. Equally it can be maintained that many of these rules are inapplicable elsewhere, and, what is more important, that technical precepts which have rightly come to be indispensable in other forms of excavating, may prove totally unsuitable to this very specialised form of practice. If one may be more explicit: to suppose that a graduate in archaeology, because he has experience of excavating an Iron Age farm on Salisbury Plain or a Roman villa in Tuscany, is thereby equipped to tackle a Mesopotamian mound, is to court disaster of the sort which even the most verbose archaeological report can never quite adequately disguise.

Now, in expressing this opinion, one is immediately conscious of venturing upon controversial ground; for, in the slightly parochial world of

British archaeological technicians, there is a school of thought which maintains the exact opposite. At present this includes some who are both great scholars and great excavators; giants of the archaeological profession, whose opinions can never safely be ignored. But in almost every case their early training has been in the field of Roman Britain. Their central loyalty has always been to the code of ethics and procedure created at the end of the last century by General Pitt-Rivers; and the theme of their teaching was the rigid discipline in archaeological practice, which they themselves have done so much to perfect. Their influence on the training of potential field-workers in this country has been prodigiously effective, and there is little in their teaching which one does not admire. In my own mind the only doubt which arises is when they maintain that the whole ritual of method—or as one might call it, the "procedural liturgy"—which they have advocated, can be applied wholesale to any excavation in any part of the world.

But already it will have become evident that the primary purpose of the chapters which follow will be to make this point clear, by summarising the conclusions to be drawn from thirty years practical experience of mound excavation. Their intention in fact is to prove that this is a specialised form of archaeology, which requires specialised training. First, therefore, it will be necessary to present what seem to be the most relevant facts regarding the anatomical character of mounds in general. After that, it should be possible to illustrate some of the peculiarities which emerge and the problems which they present, by practical instances from actual excavations. And in attempting this, it may be logical and even desirable to restrict myself for the most part to excavations which I myself have directed or with which I have been in close contact.

This then accounts for the primary theme of the book. But it has a secondary and subsidiary purpose, which I am also most anxious to make effective. For, in the category already mentioned, of handbooks dealing with the subject of archaeological method and sometimes with the history of its evolution, due to the writers' efforts to draw an effective contrast between the orderly progress of efficiency in Europe and the misguided vandalism in the past of untrained diggers in other parts of the world, less than justice has been done to some of the great figures in Near Eastern archaeology during our own time. A secondary purpose of the book then, is to recall that, in the Near East also, there have been great and methodically brilliant archaeologists since the time of General Pitt-Rivers.

CHAPTER I

Mound Formation and Excavation

In the Near East, even a peasant mentality sees in the familiar aspect of its mounds some dim relationship to the elementary principles of life and death. Alternatively their summits may accommodate the activities of village life or provide dignified isolation for a graveyard. For more sophisticated western travellers on the other hand, their silhouettes become the emblems of prolonged human survival. If their character is to be properly understood, it will be necessary first to consider how they come to exist at all; and secondly why they are to be found only in this particular part of the world. For this purpose it is momentarily essential to adapt one's mind to the peculiar conditions of life in these antique lands.

It is of course in the nature of human habitations that their prolonged occupation results in the accumulation of debris, and that, particularly if they are repeatedly destroyed and rebuilt, an elevation is gradually created which did not previously exist. But the speed and degree of this process seems to be governed by two regionally distinctive factors. One is the habits and traditions of the inhabitants and the other the form of building material which they habitually employ. Here in England for instance, there are many dwelling houses which have been occupied without interruption for a score or so of generations. A large part of my own home was built of stone in the fourteenth century and remained unchanged for more than four hundred years. But when, in about eighteen hundred, it was added to and largely rebuilt, as much care was taken to remove the resulting debris as has been taken ever since to dispose of domestic refuse. As a result, the actual level of occupation remains precisely where it was six centuries ago. Seeking a full contrast in regional conditions, my mind turns to mediaeval Baghdad. There, in 1941 I was concerned with the repair and restoration of a magnificent fourteenth century caravanserai in the centre of the town. Inside the building, occupational debris had accumulated until only the tops of the main arches were any longer visible; and this had to be removed before it could again be put into use. When the task was finished the fine proportions of the vaulted hall became apparent; but the pavement upon which one stood was now found to be exactly nine feet

beneath the level of the street outside, and a stairway had to be built in order to reach it. In a town built largely of mud brick and subjected during the past centuries to a series of appalling political and natural disasters, the level of habitation had risen at the rate of eighteen inches per hundred years. So here at once is a first clue to the regional character of mound formation; two central factors which have been conducive to their creation in the countries of the Near East. One is the almost universal employment in those countries of sun-dried brick as a building material; the other, historical insecurity, coupled with the extraordinary conservatism which makes eastern peoples cling tenaciously to a site once occupied by their ancestors and obstinately return to it however often they are ejected.

It is interesting to recollect that even Herodotus, during his visit to Egypt, was already able to observe a phenomenon caused by the accumulation of occupational debris in an Egyptian city, though his conclusion regarding its explanation was understandably at fault. In his description of Bubastis he says—"The temple stands in the middle of the city, and is visible on all sides as one walks round it; for as the city has been raised up by embankment, while the temple has been left untouched in its original condition, you look down upon it wheresoever you are."[1] In fact, as one sees today at Luxor and elsewhere, the temples, with their massive stone walls and pillars, have mostly survived at the original level of their foundation, while the surrounding dwelling-houses and other buildings of the city, whose mud and reed walls have continually been demolished and renewed, rose gradually above them, leaving them in a deep hollow, like the Forum of Trajan at Rome.

Interesting as this illustration is of how stratigraphical formations can be created, this early mention of Egypt must serve as an occasion to introduce certain reservations regarding that country, in relation to the subject under discussion. For it should be said at once that Egypt has certain characteristics which make it less suitable than others for the study of mounds. This is perhaps partly to be attributed to the abundant supply and general use of building stone, which greatly prolonged the survival of Egyptian buildings. But it is also partly due to the fact that, in the narrow valley of Upper Egypt, *land* is too valuable to allow large ruinfields of brick buildings to remain derelict; and the *fellahin* have long since discovered that the occupational debris with which such ruins are filled, when spread over their fields, makes the finest fertiliser available. But in any case, those who have approached the subject of Egyptology will know that archaeol-

(1) Herodotus, *History*. Everyman. Vol. II, 138.

ogy in Egypt, when it took the form of actual excavation, has always been concerned almost exclusively with stone temples, tombs and cemeteries. Mounds in Egypt are confined for the most part to the Delta of the Nile; and, with so much else to attend to, their excavation has till now been very considerably neglected.

So let us glance once again at the pattern of countries in which mounds are everywhere found and have been more generally excavated. From Egypt they spread northward through the Levant and westward through Anatolia to the Balkans. Eastward they follow the curve of Breasted's "crescent" through the rich farmlands in the foothills of the Armenian mountains to Iraq and Persia and so, southward of the Elburz range, to Afghanistan and the Indus valley. But the focal point of the whole area, where mounds are so plentiful that they become the most characteristic feature of the landscape, is the twin-river valley of Mesopotamia—which is in fact not a valley at all but a vast province of partially irrigated alluvial desert. It is a habit of thought to apply the name Mesopotamia to this basin of alluvium, which represents half of modern Iraq. But it has come to be known to our own generation that the first human settlers in this province, the ancestors of the later Sumerians, were themselves comparative late-comers, and that the undulating hill-country of northern Iraq had a much earlier record of Neolithic farming communities. This may help to explain the impression which has grown upon one, after long periods of travel in those parts, that the Assyrian uplands around Mosul and their westward extension through the valleys of the Khabur and Balik rivers into North Syria, must have been the most thickly populated area of the whole ancient world. Certainly today they are more thickly studded with ancient mounds than any other part of the Near East.

To confirm this, it may be interesting to quote at random the reactions of a nineteenth century traveller to the appearance of the country west of Mosul, during a journey in the spring 1840. Sir Henry Layard had reached the market-town called Tell Afar on his way to the Sinjar Hills, and he describes his surroundings as follows—"Towards evening I ascended the mound and visited the castle.... From the walls I had an uninterrupted view of a vast plain, stretching westward towards the Euphrates, and losing itself in the hazy distance. The ruins of ancient towns and villages arose on all sides; and as the sun went down, I counted above one hundred mounds, throwing their dark and lengthening shadows across the plain. These were the ruins of Assyrian civilization and prosperity. Centuries have elapsed since a settled population dwelt in this district of Mesopotamia. Now, not

even the tent of a Bedouin could be seen."[1] Layard was of course wrong in thinking only of the Assyrian nation; for many of the mounds he was looking at were in fact occupied as early as the sixth millennium B.C. But he did not exaggerate their number. During a survey in 1937, I myself recorded the surface pottery from seventy-five mounds in that area, and these were only a few selected sites which I could easily reach by car during a short three-weeks reconnaissance.[2]

But apart from the close concentration of mounds in certain areas of this sort, the pattern which they make is often worth observing. All over Iraq, and for that matter in neighbouring countries, a glance at the disposal of mounds in a landscape will often reveal to one in the vividest possible manner some aspect of historical geography, whether political or economic. The city of Erbil, for instance, (PL. 1) stands within its fortress walls on a mound whose height almost justifies its local reputation as the "oldest city in the world": and from its roof-tops, over the undulating plain to the Zaab river-crossings, which led to Nineveh and the north, one sees a line of smaller mounds, pointing the exact direction of the age-old caravan route, which the Achaemenian Persians, coming from Susa, prolonged as far as their new capital at Sardis. They called it the Royal Road, though it had existed for several thousand years before their time. Wherever it crossed a wâdi and there was a source of water, there also, today, there is a mound; and many of them are crowned by villages, which make convenient stopping places on the modern motoring road. This road of course prolonged itself through the Taurus passes, where the mounds are few and far between. But once the Anatolian plateau is reached, they start again and increase in size at the approach to the great cities of Phrygia. The crossing of the Sangarius river is marked by a colossal mound representing the remains of the old Phrygian capital, Gordion, and a wide area around it is studded with tumuli covering the graves of the Phrygian kings. Excavations by the University Museum of Pennsylvania in the side of the hill have revealed a gigantic stone gateway, from which travellers on the Royal Road must have set out on their journey northward. Half-a-mile further on, a stretch of the road itself is exposed, where it passes between the tumuli; and its fifteen-foot width of stone pavement is still perfectly preserved.[3]

(1) A. H. Layard, *Nineveh and its Remains*. I Norway 1849, 114.

(2) Published in "Iraq", Vol. V, Pt. 2 1938, p. 123ff.

(3) Happening to visit the excavations when this section of the road had just been located, I found the pavement newly cleared and, standing in the centre of it, the American director, a volume of Herodotus in his hand, from which he was declaiming the passage in praise of the Persian couriers who carried the royal dispatches from Sardis to Susa.

But it is not only on great highways of this sort that the purpose of mounds can be identified. In every major highland valley of Anatolia or Kurdistan, there, probably at a river crossing or road-junction, is a substantial mound; the market-town or administrative centre of an agricultural district, which may still be crowned by the ruined castle of a feudal landlord—the "derebey" of Ottoman times. Scattered elsewhere over the face of the valley are smaller mounds which were mere villages or farmsteads. There are mounds making obvious frontier posts, and lines of mounds sketching in the communications which served military defence systems of the remote past: and there are skeins of more recent defences, like the fortresses of Diocletian's *Limes*.[1] And finally, there are tiny, insignificant-looking mounds standing no more than a few feet above the level of the plain. And sometimes these prove to be the most important of all: for they have not been occupied for many thousands of years, and the relics of their prehistoric occupants lie directly beneath the surface.

We must now consider more closely the manner in which these artificial hills come to be created. Any of the mounds which we have mentioned in the preceding paragraphs would probably serve to illustrate the broad lines of this process: but those in Mesopotamia will perhaps serve our purpose best, since they are uncomplicated by the presence of large stone buildings and at the same time provide examples of some anatomical eccentricities seldom found elsewhere. This process, then, by which in antiquity the repeated rebuildings of human habitations on a single site created a perpetually increasing elevation, is by no means difficult to understand. The average life of a mud brick building today seldom exceeds the span of a single generation: and in earlier times, military conquest or localised raiding on a smaller scale would certainly have accounted for more frequent demolitions. Roofs would be burnt or collapse and the upper parts of the walls subside, filling the rooms to about a third of their height with brick debris. Before rebuilding, the site would usually be systematically levelled, the stumps of the old walls being used as foundations for the new. Thus, after a time, the town or village would find itself occupying the summit of a rising eminence; a situation which had the double advantage of being easily defensible and of affording an expansive view of the surrounding countryside. One remembers in this connection how the walls of the little prehistoric fortresses at Mersin in Cilicia were lined with identical small dwellings for the garrison; and each was provided with a pair of slit open-

(1) An attractive description of these is incorporated in Poidebard. *Traces de Rome dans le Désert de Syrie*. Paris 1934.

ings from which a watch could be kept on the approaches to the mound.[1]

What, then, an excavator is concerned with is the stratified accumulation of archaeological remains, unconsciously created by the activities of these early builders. By reversing the process and examining each successive phase of occupation, from the latest (and therefore uppermost) downwards, he obtains a chronological cross-section of the mound's history, and can, if circumstances are favourable, reconstruct a remarkably clear picture of the cultural and political vicissitudes through which its occupants have passed. But it must be remembered that the procedure which he adopts, itself involves a new form of demolition. For as the architectural remains associated with each phase of occupation are cleared, examined and recorded, they must in turn be removed in order to attend to the phase beneath. In a Near Eastern mound, the end-product of an operation of this sort is often a deep hole in the ground and very little else that could interest a subsequent visitor to the site of the excavation. An alternative situation arises, when an important building or civic lay-out is encountered, of the sort which may afterwards need to be preserved as an archaeological monument. In this case the excavation will merely be extended to cover as much as is required of the stratum concerned, and if a stratigraphical sounding to a greater depth is required, it will be made elsewhere. (PL. 2)

But to return to the creation and development of mounds themselves, it would be a mistake to think that the process is always so simple and straightforward as that already described. A wide variety of circumstances may serve to disrupt their symmetry and complicate their stratification. For instance, the diminishing livingspace at the summit or a sudden increase in the settlement's population may cause the focus of occupation to move away from its original centre. In order to make this clear, we may at this point enumerate some of the principal variations of the theme of anatomical development which are to be found, particularly in Mesopotamian mounds.

As a point of departure then, let us take the orthodox sequence of developments illustrated in the upper part of Fig. 1. This diagram represents the habitation of a village community with a static population. The superimposed remains of five principal occupations have gradually created a small artificial hill: but as the site of the village rose in level, the building space on the summit became more and more restricted by the sloping sides of the mound. It may well have been for this reason that the place was eventually abandoned. In any case, after the inhabitants of the fifth

(1) Described in J. Garstang. *Prehistoric Mersin*. Oxford 1953.

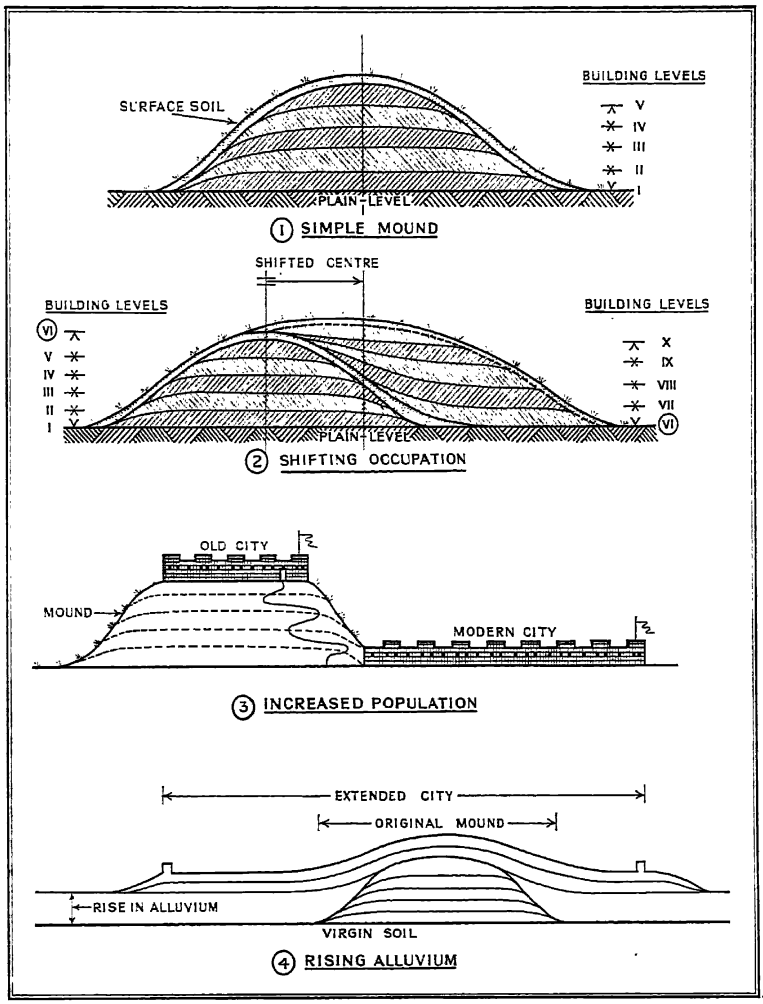

Figure 1. Diagram of mound formations.

settlement had departed, the ruins of their houses were moulded by the weather to form the peak of a symmetrical tumulus. Vegetation started to grow upon it, and soon all traces of occupation had disappeared beneath a shallow mantle of humus soil.

The second and third diagrams in Fig. 1 both illustrate cases where the focus of occupation has shifted. The former represents a phenomenon which we shall later have an opportunity of studying in greater detail at a particular site—Tell Hassuna in northern Iraq, which will provide a perfect example.[1] In the diagram, after five principal periods of occupation, a small mound has been formed in a manner exactly similar to that in the previous instance. But from this point onwards, occupation has continued, not on the summit of the mound, since that had become inadequate, but terraced into its sloping flank and spreading over an extended area of new ground beneath. Further rebuilding therefore caused the mound to extend its coverage in that direction without any increase in its maximum height. It will be seen how this sequence of events could create a typical paradox in stratification, since traces of the same occupation, (Level VI in the diagram), could be found near the summit of the original mound, or conversely at plain level in the extended sector. This is in fact one of those cases where it is easy to imagine how much wrong information could be obtained, if an archaeological sounding were made without previously apprehending the implications of the mound's formation. In the third instance, the process of extending the settlement to accommodate an increasing population has taken a much simpler course and the resulting phenomenon is one more frequently to be seen throughout the Near East. Indeed, for an example one need look no further than the city of Erbil, which we have already mentioned in another connection. Here, a tremendous increase in the importance of the city and its consequent space-requirements has occurred late in the life of the mound, so that the capacity of the original fortified city has become totally inadequate. An "Old City" has accordingly survived in a picturesquely elevated position, while a "Modern Town" has developed on the flat ground at the base of the mound.

The fourth and last diagram represents a situation of great technical interest but of some rarity, since it can occur only in riverain areas such as the Tigris and Euphrates valleys. Here, as the occupation level of the mound has risen, wind-borne dust and the silt deposited by irrigation water or floods have caused the surrounding alluvium almost to keep pace with it; and soon, the remnants of the earliest settlement are buried deeply beneath

(1) See p. 71.

the contemporary level of the surrounding plain. Meanwhile, an increase in the population has necessitated an outward extension of the city over the accumulated alluvium. And now it can be seen that, whereas the foundations of the central buildings rest directly upon the summit of the ancient mound, those in the extended area are built at almost the same level on ground never before occupied.

A final alternative may be seen in Plate 3. Here, by Roman times, the actual height of the mound (150 feet) has made life at its summit impracticable, and a village has grown up around its base.

After citing even these few instances of possible aberrations in the process by which a mound is formed, it will have become increasingly clear that the task with which a Near Eastern excavator is faced may well be an extremely complicated one. Ideally, the anatomical peculiarities of a site should be diagnosed before ever an excavation is begun; and this will mean that the archaeologist, after sufficient experience, should be capable of looking at an unexcavated mound and sizing up the sequence of developments which have created it. This of course is not always possible: but failure to attempt it may involve him in the embarrassing situation of having to reverse all his basic conclusions every time he starts a new season's digging. But with all these possible permutations of stratigraphical evolution, the central problem which confronts him will be one which varies very little from site to site. He must expect to find himself dealing with successive superimposed layers, containing partially demolished buildings, filled with the residuary material of human habitation. His first care therefore will be to articulate and record the surviving architectural remains. And so, for the first time, emphasis is placed on the all-important technique of wall-tracing: a subject about which a good deal must presently be said. Meanwhile, regarding the incidental discovery of pottery and other objects within the confines of each stratigraphical layer, these serve the purpose of dating, or alternatively can themselves be dated by the period of occupation to which they belong. It goes without saying therefore, that the whole of the digger's care and ingenuity will be concentrated on correctly determining their stratigraphical provenance.

Let us then visualise an excavator's approach to a hitherto unexplored mound, in greater detail and in approximately the correct order of procedure. We need not for the moment occupy ourselves with the preliminary identification and choice of a site, since much comment will be devoted to this in a later chapter. Also we must assume that a topographical survey has already been made, preferably with contours at every metre or half

metre, showing the mound's exact conformation. The map will have been covered with a grid, probably of hundred-metre squares, and the corners of these squares will have been permanently marked on the site. Wherever work is begun, they may again be sub-divided and smaller squares marked out, with numbers and letters for correct identification. Broadly speaking, one now has a two-fold purpose, which comprises both a vertical and a horizontal excavation. Of these the vertical sounding comes first, since its purpose is to obtain a preliminary idea of the stratigraphic situation and to determine the occupation level or period it is most appropriate or profitable to excavate.

The choice will now rest between two different kinds of sounding; and it may be said at once that each has equally formidable disadvantages. One variety, ordinarily known as a "step-trench", is cut into the flank of a mound, each step being intended to correspond to a change in building-level (PL.2). This raises no problem regarding the disposal of earth, which is merely dumped on either side of the trench. But the cutting has first to penetrate a layer of superficial debris, spilt out down the side of the mound from later occupations: and unless this is effectively discarded, properly stratified material cannot be reached. In the prehistoric mound at Mersin, (PL. 4) we spent several months making this sort of sounding, before realising that we were excavating outside the walls of a Chalcolithic fortified settlement; and did not actually get into the fortress itself until the second season. So a safer method from this point of view is to make a vertical sounding well in the middle of the mound. But this will need to have a spiral stairway or ramp around it for removing the earth; and by the time the earlier levels are reached, its area will have so much diminished as to be hardly useful. Also, one will not, at this early stage, yet know where the earth can be safely dumped without covering up something which one may later want to excavate. On the whole it is usually better, first to make a superficial examination of the whole summit by means of trial trenches, so that a preliminary idea may be obtained of the topography of the settlement beneath. And here again, in my own opinion, the very word "trial trench" gives one far too rigid an impression of the sort of sounding that should be made. It conjures up the characteristically methodical system of trenches known as *suchgrabung*, which used to be employed by the earliest German excavators in Iraq, when starting a new site. The whole area would be covered by a mathematically regular pattern of trenches, twenty metres long and five wide, with gaps between to preserve the section. In theory, these could not fail to locate any conspicuous architectural re-

mains. But today, with labour costs enormously increased, they would be prohibitively expensive and involve a great deal of work which would be likely to prove pointlessly repetitive. The truth is that the disposal of trial excavations, their area and shape, can only be determined by practical considerations, depending on inferences made from the conformation of the mound itself. And the nature of these can only be adequately explained by citing a variety of practical examples, as it is intended to do in the pages which follow.

But for the moment it may be well to return to the subject of wall-tracing and the understanding of ancient materials.

In the very early stages of community life in the Near East, walls were often built of *pisé*, which is the equivalent of the South American term, *adobe;* that is, simple clay mixed with straw and built up in convenient lumps or slabs. After this came the almost universal use of sun-dried mud bricks, prismatic in shape but of widely varying dimensions. Mud brick, as is now fairly generally known, is made with the aid of a four-sided wooden mould, having no top or bottom. Into this the tempered clay is dumped and the surplus normally smoothed off with the side of the hand. The mould is then lifted and the brick left to dry in the sun. It is this concluding process which sets a geographical limit to countries in which mud bricks can be used, since cloudless skies and hot sunshine are indispensable to their manufacture. In almost all countries of the Near East such conditions are favourable during at least a part of the year, and up to comparatively recent times, kiln-baked bricks have consequently been considered a luxury. It is for this reason that today, in those countries, a proper understanding of the nature and uses of this material, particularly in Iraq, has become as indispensable to a twentieth century excavator as it was to the architects of antiquity. In neighbouring countries where stone is available, a wall may have stone foundations or even be built up to a height of several feet in stone before the brick begins. In Anatolia particularly, the structure above this may be a framework of wooden beams, forming panels which are filled with mud brick. In all cases the wall is finished inside and out with a plastering of mud and straw. Outside at least, this has to be renewed every year.

In the early days of Near Eastern excavating, mud brick walls were simply not understood and in some cases their very existence was hardly suspected. The normal practice was to go on digging until one reached the stone foundations, or even, in the case of Assyrian public buildings, the upright stone slabs with which the lower part of the walls were revetted.

There are still pictures which have become familiar in every textbook on excavation, like those of the Hittite palace at Sakjegeuzi, where mud brick walls six feet thick have been laboriously cut away and removed, leaving only the sculptured slabs standing, which had adorned their faces. A French excavator in Babylonia, as late as the nineteen-thirties, complained of finding no walls at all: and even in the architectural records of some Greek excavations, the supposed stone walls sometimes appear too thin to have been anything but the foundations for mud brick upper structures, the remains of which were never recognised by the excavator.

It was German archaeologists in Iraq at the beginning of the present century—Walter Andrae, Robert Koldewey and others, working at Babylon and Ashur, who discovered and perfected the technique of wall-tracing and indeed devised a routine of other specialised practices adapted to the requirements of Mesopotamian excavating. Furthermore, it was they who first set about training a group of Arab craftsmen who could attend to their manual operation. These men came at first, as in Egypt the "Qufti" craftsmen do, from one particular village, Sherqat, near the site of Ashur. And it was from a small remnant of Sherqati workmen, by then elderly men, that I myself learnt the craft of wall-tracing when I first went to Iraq in 1929. Together we taught younger men and the "guild" which we created was afterwards greatly expanded by the Iraq Antiquities Department when they began to undertake their own excavations in the late nineteen-thirties. Today the Sherqati workmen are still an indispensable asset to foreign expeditions, including that from the British School in Baghdad which has recently been excavating at Nimrud.

The tools which wall-tracers use have varied in time and still vary from country to country according to local practice. Sir Leonard Woolley, for instance, because of the circumstances under which he started excavating in Iraq at the end of the First World War, used to prefer the ordinary army entrenching-tool. The Sherqatis use two picks—one a small, single-pointed implement with a fine balance for tracing, and the other an ordinary double-pointed pick-axe for heavier work. In Anatolia, pickmen use one of these for preliminary work, a flat shovel for scraping and a long-rigid knife for finer work. Then there is the matter of earth disposal. In Anatolia, the gang which supports the pickmen, consists partly of men with long shovels, which can be used if necessary for throwing earth up from an excavation to a remarkable height; and partly of men with wheelbarrows or working a Decauville railway. In Iraq the earth used to be carried away in straw baskets, which were filled by a shovel-man attendant on the wall-

tracer. But today it is done much more economically with a kind of bag of coarse material, looped at one end round the neck and thrown over the shoulder. There are many other methods, including donkeys with pannier-baskets in Syria, or even small "kamyons" which are used by the French in Asia Minor. The economy of an excavation depends on such devices, but its effectiveness depends entirely upon the skilled digger, whether he be a trained local craftsman or the archaeologist himself.

To turn then to the actual process of wall-tracing: first, a wall-face must be located, and this is done by scraping the surface of the ground vehemently, either with a dragging tool of the hoe type, such as is used in Egypt and Iraq, or with a shovel as is more usually done elsewhere, until the loose soil is removed. Under certain circumstances the actual pattern of the brickwork then appears, but more often one can see a difference in texture and colouring between the wall and the filling; and the two are separated by a clear line of mud plaster. The pickman then cuts down into the filling until he has a hole in which he can squat, in order to approach the wall-face horizontally. The strokes of the pick or knife, with its point towards the wall, then become increasingly delicate, until the last crust of filling flakes away of its own accord, leaving the plaster almost undamaged. This process is continued progressively from wall to wall until the chamber or compartment is almost completely encircled, leaving what modern builders call a "dumpling" of filling in the centre. The trench thus created will, if possible, have penetrated only to within six inches or so of the original pavement level, and the "dumpling" will now also be removed down to this level. The last few inches of filling, in which objects are likely to be found lying upon the pavement, is finally removed by the most skilled workmen, using the more sensitive knife rather than a pick. All objects are of course preserved in their exact setting for photography and recording. As for the all-important section of the filling, from which the history of the building is to be reconstructed, this will already have been recorded by leaving a clean vertical face to the "dumpling" and occasionally linking it to the wall-face. The process just described is continued from room to room until the whole building is exposed. It must then itself be planned and photographed (PL. 5).

It is well to remember that a building may have been more than once re-paved, and that several rises in floor-level may have occurred during a prolonged occupation. It is not unusual therefore, to find a number of superimposed pavements of brick or tamped earth, at intervals of a few inches. Each of these must of course be removed in turn and the finds as-

sociated with it carefully segregated. At the base of the wall a trial pit is sometimes sunk, in order to ascertain the depth and character of the foundations. If, as is so often the case with mound architecture, this leads to the discovery that an earlier "version" of the building exists beneath, the next step will repeat the whole procedure at a deeper building level. (PL. 6) For this purpose, after a full recording, a process of demolition takes place: and it is often necessary to restrain the enthusiasm with which this task is approached by workmen who have long been subjected to caution and restraint: otherwise much may be lost.

Before concluding this much simplified summary of technical procedure, it may be well to mention various phenomena which are at first likely to be puzzling. They are illustrated diagrammatically in Fig. 2.

First then, at the base of this diagram, the plastered walls and pavements of a building appear in section. After the destruction or abandonment of this building, the site has been levelled preparatory to rebuilding: and when this came to be done, it was found that the stump of one old wall could conveniently be used as seating for the foundation of a new one. During the life-time of this second building, rises in the floor-level of the building twice necessitated the replastering of the pavement. The occupation is therefore divided into three sub-phases which are numbered accordingly. Also it can be seen that, during the third and final sub-phase, it became necessary, first to replaster the walls and later actually to reface them with brickwork. This refacing naturally does not descend beneath the third pavement level. This building, then, itself is eventually destroyed and a third is constructed with walls on a slightly different alignment. After an occupation again divided into three sub-phases, this also is abandoned: and now an interesting development takes place. The abandoned shell of the third building is to be converted into a solid platform on which a fourth building of some importance—probably a temple—will be raised up to an imposing height. For this purpose the spaces between the old walls are filled in solid with mud brick. But the fourth building is either for some reason never built or its remains have been obliterated by long exposure to the weather. In this case therefore, the architectural remains directly beneath the surface of the mound will present themselves to the excavator in the form of what appears to be an uninterrupted expanse of solid brick masonry. If, in this case, after scraping and examining the surface, he is unable to detect any coherent pattern of walls, it will be necessary for him to resort to an expedient devised by the German excavators at Warka in South Iraq and later perfected by the Americans at Khafaje.

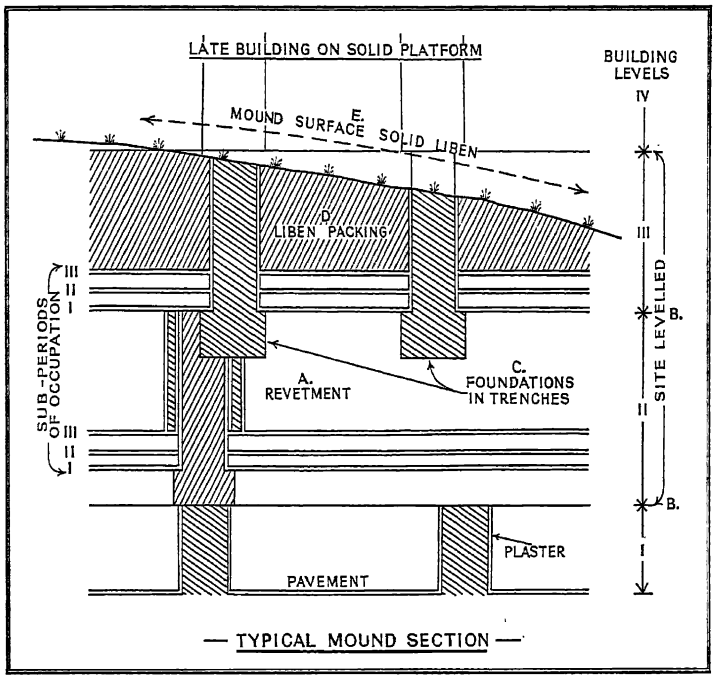

Figure 2. Diagram of building levels.

This requires the most skilled workmen; for it consists in articulating every individual mud brick by the use of a sharp-pointed knife and brush. Foundations, packing, etc., can then usually be distinguished by studying the bonding and texture of the bricks and the variations in their dimensions at different periods.

And so, the process of stratigraphical excavation continues, until one finds oneself repeating the routine of wall-tracing and recording, perhaps for the twentieth time, at a depth of anything up to a hundred feet beneath the surface. (PL. 7) This is the moment when the appearance of the excavation becomes most puzzling to the layman's eye. So often one has heard the question asked—"But why did these people prefer to live underground?". In the Near East this sort of enquiry is usually made by local peasants. But it is really no more grotesque than the comment which I once heard made by an English-speaking visitor to the Acropolis at Athens. Stamping a little with his feet, he said—"What puzzles me is to know how they managed to get all this *rock* up here." The two comments are really complementary misconceptions.

It would be a mistake to end these observations on procedure without once more emphasising the most essential requirement in this type of excavating. Some appreciation has already been expressed of the skill which locally born craftsmen now bring to the aid of the foreign excavator in the Near East. But it should also be considered as axiomatic that the excavator himself, who conducts or supervises excavations of this sort, should in turn be completely proficient in the skilled tasks for which he employs them; so that he himself may move from trench to trench demonstrating and correcting their work. The importance of this actual participation by the excavator in the manual tasks of the excavation will, it is hoped, become increasingly clear in the course of subsequent chapters. For the narrative which follows will be composed largely from personal experiences in this capacity.

CHAPTER II

Mesopotamian Methods

The two great centres of Near Eastern archaeology, in which British explorers and scholars have in our time played a leading role, are most obviously Egypt and Mesopotamia. And though it was in Egypt that my own field experience started, as long ago as 1929, it is equally certain that, in our search for practical illustrations of the problems discussed in the last chapter, Mesopotamia will prove the more profitable point of departure. Some characteristics have already been mentioned which distinguish the common forms of digging in the Nile valley; clearance of tombs or stone buildings and comparatively infrequent excavation in mounds or other stratified sites. Instances are in fact hard to find there of operations which have a direct bearing on our present subject; and it would probably be true to say that, even among Egyptologists with wide field experience, there are those who have never themselves encountered the necessity for self-dedication to these same propositions. In Iraq on the other hand, during the past fifty or sixty years, a specialised technique in the excavation of mounds has been evolved empirically by archaeologists of several nationalities; and it has been their work which has tended, where method and approach were concerned, to differentiate this particular discipline from all others. Let us then, for the moment turn from the Nile valley to the twin rivers of Iraq, and try to recapture the peculiar atmosphere of the mounds, great and small, whose existence depends upon them and upon their flood waters. Memory of their versatile individuality is still fresh in one's mind; and there should be little difficulty in reconstructing a coherent sequence of professional experiences in their excavation. This may prove an effective way to present in miniature the long and complicated story of how, during the nineteen-thirties and forties, a "Mesopotamian technique" was evolved—a technique which, refined by new and ingenious accretions, is being used to good effect by young archaeologists in Iraq today.

In speaking of the system by which this technique was developed in its early stages, I have used the word "empirical", for it would seem to be an appropriate one. The Oxford Dictionary defines it as—"based or acting on

observation and experiment rather than theory"; and that in this case seems exactly applicable. One might perhaps alternatively use the phrase, "trial-and-error"; but in that case one would claim that such errors as there were, occurred in the early stages only and were soon corrected. For it is necessary to bear in mind the general situation in field archaeology at the end of the second decade of the present century. It was not possible in those days to learn how to excavate a mound from textbooks or university courses. One could profit to some extent from the mistakes made by one's predecessors in the field, as far back as Schliemann or even Layard. One could learn something from the meticulous reports of the German excavators at Babylon and Ashur, (strangely inarticulate as these were when any explanation of practical expedients was concerned). One could, in addition to the Germans, visit and see excavations which had been started since the first German War by British, French and American archaeologists, each with its own complement of improvised expedients. There were the American expeditions, with their multiple card-indexes and photographic kite-balloons, often seeming to be involved in trying to apply a kind of prefabricated methodism under obstinately unsuitable circumstances: British expeditions, usually under-subsidised and dependent on the popular interpretation of their finds to obtain funds for the continuation of their work: and French missions, still curiously intransigent, inspired by Champollion but clinging to the methodical dogma evolved by de Morgan at Susa.

There was a limit to the amount one could learn from all these. Admittedly it was possible at some sites even for an inexperienced eye to see how the technical inadequacies of the actual digging could impair the logic of the excavator's conclusions. But at others, little could be learnt at all, either about technique or about logic, since both the strategy and the purpose of the various operations seemed to be an esoteric mystery, whose understanding was the exclusive prerogative of the mind directing the excavations. The field-staff were then mere acolytes, each with his appointed routine of practical duties. This is not intended as disparagement of what others had already done. It is no more than an attempt to make clear that, to anyone arriving in Iraq in those years, with the task of organising and conducting a large-scale and long-term excavation, there was little alternative to an empirical approach. And this was precisely the situation which faced the late Henri Frankfort, whom I accompanied there in 1930, when he was commissioned by Breasted to inaugurate an "Iraq Expedition" for the Chicago Oriental Institute.

Certainly, when we arrived in the spring of that year, we came with open minds and very little idea of what would be required of us. Frankfort's experience had been entirely in Egypt, where he had first worked as a pupil of Petrie. After that, he had excavated cemeteries at Abydos and Armant; and finally, with myself as architectural assistant, he had been field director for the Egypt Exploration Society at Tell-el-Amarnah. Now, if ever there was an elementary and undemanding form of excavation, it is the clearances which have been made by British and Germans in Akhenaten's short-lived capital at Amarnah. It was built, occupied and abandoned all within a space of about twenty-five years; and it survives today as twenty miles of ruins along the banks of the Nile, occupying a site of great natural beauty, protected by a great semi-circle of cliffs. The mudbrick walls of the buildings remain standing, often remarkably well preserved, up to a height of five or six feet. The rooms are filled with the fallen debris of the upper structure and the whole of the ruins are lightly drifted over with sand. It rains at Amarnah approximately every ten years: so everything under the sand is beautifully protected by the dryness of the climate—even the painted frescoes on the plaster of the walls. There is little stratification to elucidate, and almost all the pottery and small objects which one is likely to find have long ago been classified by previous excavators. (PL. 8)

For two winter seasons then, my duty had been to supervise an extremely competent gang of Qufti workmen, while they raked away the sand and dry debris from the floors of the rooms, and then to survey or plot the plans of the houses with a view to making scale-models, which afterwards made very entertaining museum exhibits. One eighteenth dynasty nobleman's house in the north suburb provided quarters for the expedition. It had only been a matter of raising the walls a few feet and roofing them over with palm branches to make it habitable. Beneath this, the original pavements were covered with clean sand and camp furniture arranged between the huge circular column-bases. It was a very pleasant place to live in indeed: but my sojourn there had taught me very little about archaeological method.

Curiously enough, when we came to start work in Iraq, the ruined city of Khorsabad, which Frankfort had chosen as the scene of our first tentative experiment in Mesopotamian archaeology, had something in common with Amarnah, in that it also had been built and occupied by a single generation only. One even suspected that Frankfort himself had borne this fact in mind: for we were a very inexperienced party and might well have found the complicated stratification of a more normal mound beyond our

capacity to deal with. Let us then consider the circumstances with which we were faced at Khorsabad.

The site lies on a small tributary of the Tigris, fourteen miles north-east of Nineveh, and its history in the records of archaeology began in 1843, when Emile Botta, then French Consul at Mosul, had recently begun excavating at Nineveh itself and had, rather surprisingly, so far met with very little success. One of his workmen drew his attention to the mounds at Khorsabad, and, in the manner of excavators at the time, he "put a gang of men to work there", visiting them every few days to check their progress. After a week's work, it became clear to him that what he had discovered was, to use his own words—"a huge Assyrian palace, containing a large number of chambers and corridors all the walls of which were lined with slabs, having sculptured representations of gods and kings, and battles, and religious ceremonies. Side-by-side with these representations were long inscriptions in the cuneiform character." In fact, he felt justified in sending off to the Louvre his famous dispatch, simply saying—"Ninive est retrouvé." It was not of course actually Nineveh, but the palace of King Sargon II of Assyria in the city of Dur Sharrukin, which he built as his new capital during the fourth quarter of the eighth century B.C.

At Khorsabad we were living in an empty farmhouse built by Kurdish villagers on the highest part of the main mound. From the roof-top one could see the whole conformation of the city's ruins; an enclosure just under a mile square, surrounded by mud brick walls eighty feet thick, with seven gateways, most of them ornamented originally with winged-bull portal sculptures. Where one stood, level with the centre of the city on the north side, a vast platform of solid mud brick had been raised to the full height of the walls, no doubt taking advantage of a more ancient mound which already existed at that point. It was upon this that Sargon built the great palace which, with its royal apartments, private temples and miniature *ziggurat* tower, forms the basis of the familiar reconstruction afterwards made by the French excavators.[1] This building, on its platform, accounts for the main mound at Khorsabad on the summit of which we lived. Smaller mounds covering the ruins of the gateways and the line of the city walls themselves can be seen spreading out into the cultivated plain on either side. It was one of these gateways—No. 7 in the plan made by Botta's successor, Victor Place, in which we started excavating in 1930.[2]

(1) This is now incorporated in the American expedition's fine perspective reconstruction of the whole palace setting. G. Loud. *Khorsabad*. Vol. II. Frontispiece.

(2) Reproduced in G. Loud. *Khorsabad*. Vol. I. Chicago 1936. Fig. 1.

We pegged out a careful trench across one side of the little hill, and, as might have been expected, dug for several days without finding anything at all.

At about that date our party was joined by Mr. Pierre Delougaz, who is now Curator of the Oriental Institute Museum, and afterwards it was not difficult to discern that his arrival marked the beginning of our seven years experimenting and discovery in the realm of excavating technique. In fact, so much of the effective work referred to in the remainder of this chapter must be credited to Delougaz' insight and initiative, that it may be well here to explain his presence at that time in Iraq.

In the previous winter of 1927/28, Breasted's Iraq Expedition had suffered what could be regarded as an unfortunate false start, in that Dr. Edward Chiera, who had been in charge of it, had unhappily died almost before it had time to get under way. Chiera himself had at first concentrated on the Khorsabad palace. He had arrived to find the site pillaged and neglected, with everywhere signs of the looting and destruction which had continued throughout the long aftermath of Botta's and Place's excavations. The place had to all intents and purposes become a stone-quarry, from which the sculptured slabs were extracted to be broken up and burnt into lime for local building purposes. There was then a village on the summit of the main mound, and he noticed in the courtyard of the local *agha's* house a fine bearded head of one of King Sargon's officials, retained as a curiosity and now being used as a chopping-block for wood. The sight of such vandalism was—as Chiera with remarkable restraint observed in his report—"irritating to say the least", and he spent the remainder of his first season in effecting such rescue-work as he could manage; packing and removing the surviving slab fragments in several of the principal chambers. (PL. 9) This in the end involved him in what proved to be an almost embarrassing discovery—the broken pieces of one of the largest portal sculptures of all—a winged bull from the entry to Sargon's throne-room, which now stands in the Oriental Institute Museum.[1] The largest fragment of this sculpture weighed rather more than fifteen tons, and at the end of the season it was Delougaz who was left with the task of transporting this and all the other sculptures fourteen miles across country to a river-steamer on the Tigris at Mosul. (PL. 10) The adventures of this lift have been described elsewhere; and its only relevance in this context is to explain how, during the many weeks which Delougaz spent in extracting and removing these architectural remains, he was able to obtain an intimate knowledge, both regarding

(1) Loud. *op.cit.* Vol. I. Fig. 56.

the fabric of mud brick buildings and the technical capacity of local labourers.

All this goes some way to explain how, when he joined the same expedition under its new leadership in 1930, Delougaz was able to start by pointing out that the reason why our newly dug trench seemed innocent of buried antiquities, was because it was cutting into the fabric of a solid brick wall, eighty feet thick. After this, he demonstrated to us for the first time the process described in the previous chapter, whereby one scrapes away the surface soil over a wide area, until the face of the wall reveals itself as a thin line of plaster, and on either side of it the texture of the mud brick can then be distinguished from that of the filling. In a few weeks we had succeeded in clearing, with passable neatness, the ruins of a city gate, and had also the satisfaction of observing that we had not, like most of our predecessors, removed the brickwork from behind the facing-slabs.

And now the more serious work of the expedition began. Frankfort decided that we were competent to tackle Sumerian sites in southern Iraq and our party was split up into three parts. Gordon Loud remained at Khorsabad and, during the following three years, cleared the vast complex of temples and other buildings at the foot of the palace mound, of which Botta and Place had not suspected the existence.[1] In the south, we obtained a concession to excavate a group of Sumerian cities in the neighbourhood of the Diyala river, east of Baghdad, and erected ourselves headquarters at the largest of them; Tell Asmar. At Tell Asmar, Thorkild Jacobsen and myself took charge of separate sections of the excavations. Delougaz meanwhile was given the site called Khafaje, eighteen miles away on the banks of the Diyala. We had now equipped ourselves with half a dozen skilled Sherqati workmen from the German excavations at Ashur, and, though these were now of an advanced age, we were able to learn from them and even improve upon the German technique, sufficiently to instruct a much larger gang of younger men.

During the seven years which followed, our expedition excavated two further mounds in the Diyala area; Tell Agrab and Ischali,[2] and during the nineteen-forties the Iraq Antiquities Directorate investigated two more; Tell Harmal (PLS 13, 14) and Tell Mohammed.[3] Of these sites Khafaje was un-

(1) Loud. *op.cit.* Vol. II. Frontispiece.

(2) Ischali was excavated by Professor T. Jacobsen. A magnificent reconstruction of the Ishtar-Kititum temple by the late H. D. Hill appears in H. Frankfort's *Art and Architecture of the Ancient Orient.* Penguin Books, 1954. Excavations at other Diyala sites are described in P. Delougaz and S. Lloyd *Pre-Sargonid Temples in the Diyala Region.* Chicago 1942.

(3) Preliminary reports by T. Baqir in "Sumer" Vol. II 1946 and Vol. IV 1948.

doubtedly the most remarkable archaeologically and also the most productive. One feels in retrospect that, during his operations there, Delougaz was faced with and brilliantly solved almost every problem which could face the excavator of a Sumerian mound. We had been directed there in 1930 by Sidney Smith, who was then Director of Antiquities in Baghdad, because it was being illicitly exploited by the local antiquities dealers; and the scores of whole and fragmentary Sumerian statues which they were offering for sale already exceeded the total of such sculptures in the museums of Europe and America. The site itself, when we first visited it, looked like a battlefield. The main mound on the south-east side was completely honeycombed with holes large enough to be shell-craters, surrounded by mountains of discarded earth, still mixed with a scattering of statue fragments. It was a sight which might well have daunted the most experienced excavator: for here, quite clearly, none of the conventionally recommended methods of approaching an excavation could reasonably be applied. Delougaz rightly decided that, before any further step could be taken, the whole of these robber excavations must be completely cleared of loose soil. Accordingly, for the entire duration of his first season, one half of his available labour was concentrated on this formidable task, which left him free, with a small and more manageable gang, to experiment with an undamaged part of the site. He chose an open area on the western side of the main hill, where preliminary soundings had already been made by Conrad Preusser, a member of the Deutsche Orientgesellschaft expedition at Warka.

Preusser, working there in the previous spring, had adopted the exactly prescribed procedure that came naturally to him after his long training with Andrae and Nöldeke. His first task was to make a topographical survey of the site: and for this purpose it was correct to make a sounding, in which a datum-level could be fixed, either at a point where clean soil was reached or at the watertable. In this way it was also presumable that one could obtain a preliminary idea of the stratification and the location of any important buildings. His metre-wide trench did in fact accomplish the first of these purposes at a depth of some twenty feet beneath the surface. But after the first eighteen inches, he was astonished to find himself passing through almost clean sand, with no trace whatever of archaeological remains to be seen. This was beyond his understanding; and in fact was not explained until some years after Delougaz had taken over the excavation.

In the meantime, a proper examination of the trench in section showed that, over its whole length, just beneath the surface, it had cut through

the foundations of a very large building which remained standing hardly more than a few inches high. It was clearly time to adopt the second step in the schedule of German procedure, and work carefully over the surface with trained wall-tracers. This Preusser had done, and before he left the site, had succeeded in exposing the outline of a mud brick enclosure-wall, twelve feet thick, making a wide curve around some central structure. It was at this point that Delougaz took over. He decided that Preusser had been right in adopting the Warka technique of working horizontally over the surface—not relying on the tell-tale line of plaster to show the face of the wall, since in some cases he was dealing with unplastered foundations, but cleaning and articulating with the point of a knife every single mud brick, so that the shapes of the walls might reveal themselves. Now it can be imagined that economical and other considerations made this a difficult decision to make, since a superficial area of about ten thousand square yards would have to be covered in this way, with much consequent loss of time and very little certainty of tangible results. For the workmen also it would involve a long course of instruction in this new craft. The sun-dried bricks of which the structure just beneath the surface was composed were of the so-called "plano-convex" variety, which are in fact characteristic of almost all late Sumerian architecture. And in the weeks which followed Delougaz set himself to make an analytical study, both of their composition and of their structural use, and the results which he subsequently published made an important contribution to historical technology.

Being familiar with the extreme conservatism of all peasant crafts in the Near East, Delougaz started by studying the process of brick-making as practised by the villagers at Khafaje. He saw how the best clay for the purpose was found near the surface in close proximity to an irrigation canal, and how the area in which the bricks were to be made expanded itself along the banks of the canal, in order to have water readily available. He saw the chaff tempering being trodden into the wet material and a bed of chaff or dry sand prepared on which the clay could be handled before filling the wooden mould; for the selected lump of clay had first to be kneaded and rolled like pastry. The final gesture was the scraping of surplus clay off the top, and the bricks were afterwards placed separately to dry. The operation usually took place in the spring, after the last rains, and the bricks then dried throughout the summer, ready for building operations in the autumn. Next he was able to distinguish the technical idiosyncracies by which the plano-convex bricks were differentiated. The mould for these

seemed to have been hardly more than two inches deep but the surplus clay, instead of being scraped off level with the top of the mould, was merely pressed down and rounded with the hands, giving the brick a convexly curved upper surface, rather like a cake baked in a tin. Closely examining the section of these bricks, he could even detect a spiral vein of chaff-dust, caused by flattening and rolling the lump of clay in the hands. Finally each brick had received on its upper surface the imprint of a thumb or other part of the hand, perhaps as the distinguishing mark of the brick-maker.

Later also it became interesting to observe the manner of laying plano-convex bricks in the fabric of a building. The normal bonding of modern bricks—"English" and "Flemish" for instance—depends on their regular prismatic shape. Plano-convex bricks were much less satisfactory when laid flat; and the normal method seemed to be to lay them "on edge" at a slight angle, each one leaning against the next and the courses leaning in opposite directions, so that a "herring-bone" effect was produced in the wall-face. (PL. II) At corners of buildings or lining any openings in the walls, small piers had to be constructed of bricks laid flat, in order to give stability; but between these, the herring-bone filling only needed an occasional flat course to keep it in place. All these details Delougaz carefully observed and recorded,[1] because to him it was already clear that collectively they had a special significance, capable of promoting this quaint system of building from a mere archaeological curiosity to an invaluable dating criterion. For, as it afterwards proved, it was a device only employed during the comparatively short period of three centuries in the early third millennium B.C.: and consequently, wherever the tell-tale evidence of herring-bone brickwork in the face of a wall appeared, the building could be confidently dated to the third phase of the Sumerian Early Dynastic period.

But to return to Delougaz' operation at Khafaje; his intimate knowledge of plano-convex bricks now enabled him without difficulty to master the technique of articulating them *in situ* with the point of a knife. So he proceeded to instruct a class of young workmen, who could from then onwards be seen working in a row along a marked strip of ground, leaving a mosaic of cleaned bricks behind them. Finding at one point that his pupils tended to *invent* bricks, he took to marking a serpentine trench, so that aberrations of this sort could immediately be detected. And since then, inevitably it has sometimes occurred to one to wonder how the more rigid disciplinarians of the Roman-Britain school would have regarded the sight

(1) Delougaz in "Studies in Ancient Oriental Civilization" No. 7 Chicago 1933.

of a trench without the regulation "balks"—wandering in an irregular meander across an archaeological horizon.

So, from all this work there gradually emerged the plan of the so-called Oval Temple at Khafaje, dating from the third Early Dynastic phase of Sumerian history in about the twenty-sixth century B.C. The architectural drawings show it to have been an oval enclosure, lined with chambers which bring the inner courtyard to a rectangular shape. Inside this courtyard at one end is a rectangular platform, approached by a stairway, upon which the sanctuary itself was raised—a miniature version of that upon which King Sargon's palace stood at Khorsabad. In this case there could be no question of reconstructing the appearance of the sanctuary, since of the platform, which was made of solid mud brick, only two or three courses remained standing. This also applied to the whole lay-out, including the outer wall, parallel to the enclosure, which was of irregular shape, enclosing a rather substantially built dwelling-house, presumably for a chief priest. But for the moment the physical remains of these buildings presented a picture more closely resembling a wide expanse of cobblestones, in the pattern of which the plan could just be distinguished. (PL. 12)

With the whole structure thus denuded, often to beneath its original pavement level, it was remarkable that such a comparatively large number of antiquities could have been recovered from undisturbed corners or subpavement recesses. They included a group of three magnificent bronze statues, one of which, almost half life-size, is now in the Iraq Museum. Equally remarkable was Delougaz' recognition among the surviving fragments of brick work, of structures dating from three successive building periods and many other details which he was able to recover. He had now perfected the craft of brick-cleaning and some ingenious devices had been added to the equipment of the Sherqati workmen who were engaged upon it. The process of picking with a knife-blade and then using the human lungs to blow away the chippings or dust, had proved exhausting. He had therefore tried substituting a jet of compressed air from a tube attached to a cylinder. This meant that very fine work could now be done, and, after completing the plan, he turned his attention to the clay pavement of the courtyard in front of the platform, where there were rows of low pedestals for offerings. Sheep had evidently been sacrificed here; for everywhere he was able to expose their hoof marks in the damp clay. At one point he was even able to point out to me the marks of a particularly large animal, probably a ram, which had been held by a rope: for behind it, unmistakably, came the slithering footmarks of a man trying to hold it. This evidence

could be photographed, and plaster casts were made of the hoof-marks for identification.

Delougaz was also able to make some ingenious reconstructions of structural details in the vanished buildings, especially the priest's house, which was slightly better preserved owing to its protected position between the two enclosure walls. In the published evidence, it can be seen that the key to his reconstruction of the ceiling is not the fallen beams themselves, for these had entirely perished, but a large wasps' nest built among the rafters, whose cellular structure of clay miraculously preserved an impression of them in negative. (PL. 15) Actually, the total decay of the structural woodwork seemed a little curious; because other more fragile substance had survived. Some member of the priest's household for instance, had evidently been in the habit of fishing in the Diyala. For in one room there was a great pile of circular clay net-weights, and between them enough of the net itself survived to reconstruct its mesh.

And finally Delougaz applied himself to the problem of what lay beneath the oval. He then discovered that the findings obtained from Preusser's trench were substantially correct. The entire temple complex, up to the periphery of the outer enclosure wall, was founded on a bed of clean sand having a depth of over fifteen feet. He was by now aware that this could not be a natural deposit, since, in a neighbouring part of the site to which I shall presently refer, he had continued to encounter occupational levels right down to the water-table, twenty-seven feet beneath the surface. He was able to check this situation by cutting a careful section through the outer enclosure wall. This showed very clearly how the horizontal strata of occupation levels, (previous to the temple period), stopped dead against the sand deposit, of which the outer face could be traced sloping sharply inwards as it descended. For some reason—perhaps for some ritual purpose, about which there is much speculation in Delougaz' report—the site on which the temple was built must have been excavated, perhaps down to the clean soil beneath, and then filled with uncontaminated sand brought from outside the city. He estimated that the cubic capacity of the excavation made to receive the sand and therefore of the sand itself was sixty-four thousand cubic metres.

To us this may seem an unreasonable and pointless task for a builder to have deliberately set himself. But the impulse to seek a basis of clean soil for the foundations of a monumental building is something quite frequently found in other periods of Mesopotamian history. An even more striking example is for instance Nebuchadnezzar's Babylon. There, as is

today fairly well known, the king applied this principle to the whole vast structure of the famous Ishtar Gate and the walls of the so-called Procession Street behind it. Modern visitors to the site of Babylon see the walls of these enormous buildings still perfectly preserved to a height of about thirty feet, and ornamented with heraldic devices of dragons and bulls, modelled in relief on the brickwork. And it is difficult for them to realise that these are in fact not the walls at all, but merely the foundations. The gateway and walls themselves, which were identically ornamented—but in brilliantly coloured glazed brickwork, stood on top of the present ruins and have now almost completely disappeared. In order to reach uncontaminated soil, Nebuchadnezzar had cut deep trenches down through the remains of earlier cities, and built in them these foundation walls, whose decoration was never meant to be seen by human eyes. In fact, the relief figures had been carefully plastered over with a protecting coat of clay before the trenches were filled in.

To return to Khafaje, throughout its excavation Delougaz had felt a great deal of curiosity regarding the *oval* shape of the temple enclosure: and one line of enquiry in this respect had led a few years later to a test excavation elsewhere which will be described in a later chapter. But for the present he had finally become involved in the excavation of the main Khafaje mound, where, during his first season, a large gang of men had been almost continually at work, simply clearing loose earth from the holes made by illicit diggers. By 1933 this work was finished and among the extraordinary honeycomb of plundered mud brick buildings, it was now possible to assess the amount of damage that had been done and to set about recovering such evidence as was not permanently defaced. In fact, Delougaz was able in his report to sum up the situation in comparatively optimistic terms. "The very irregularity", he says, "with which the robbers had conducted their digging proved rather an asset, for finally we were able to obtain sufficient evidence from the intact remains between their holes for the reconstruction of the plan of even the latest building, immediately beneath the surface of the mound." And it is interesting to notice that, while on the subject of illicit digging generally, he adds—"In the writer's opinion, the loss of archaeological evidence by such damage is seldom irreparable, in contrast to that caused by some so-called 'scientific' excavations, whose very thoroughness may result in irretrievable loss if inadequate methods or insufficiently experienced workmen are employed."[1] Undoubtedly these strictures are borne out by Delougaz' plan of the build-

(1) P. Delougaz and S. Lloyd, *Pre- Sargonid Temples in the Diyala Region*. Chicago 1942. p. 3.

ing directly beneath the surface, which the work of his now highly skilled wall-tracers enabled him to recover. This was the famous Sin Temple of Khafaje, from which such a tremendous wealth of Sumerian sculpture was obtained: and, having identified the building by the inscribed objects found in it, he now proceeded to do what had once long ago been done by the German excavators in the Ishtar Temple at Ashur: he excavated and recorded no less than ten major rebuildings of this temple also, right down to an original foundation on clean soil almost thirty feet beneath.

The excavation of the Sin Temple at Khafaje set a standard of technical proficiency for all times. (PL. 16) The designation of the successive strata show that this was one of those cases where, an archaeological section having been completed down to virgin soil, a rearrangement can be made of the terminology used during the excavation. The numbering of the strata accordingly reads from the bottom up; "Temple I" being the original foundation, "Temple 2" the first rebuilding and so on. Delougaz applies these numbers to the architectural remains themselves and also to the so-called "building periods", during which each building was actually in use. In his system of terminology, within a single building period there may be a number of different "occupation levels", at each of which modifications of the building have taken place. In the life of the Sin Temple, he recorded twenty-four of these sub-periods and he points out that they again could be sub-divided chronologically by the re-laying or replastering of the floors, which could hardly be recorded in detail as it had taken place several hundred times.

As the excavation descended, the walls of each successive temple were planned and recorded, and then completely removed in order to isolate the stratigraphic setting of objects in the temple beneath. Only the outer enclosure walls were left standing, partly to give a firm vertical face to the excavation and partly because, by articulating their brick bonding and marking the lines of successive pavements, they remained as an *in situ* record of the archaeological section, exactly as "balks" do in a conventional western-type excavation. The Sin Temple excavation continued in this way down to Temple 3. After that, its area had to be reduced owing to the saturated condition of the soil; but beyond this area, the skill of the wall-tracers sometimes enabled them to extend the plan by tunnelling along the wall-faces. In Temple 1 this procedure became dangerous owing to the seepage of water from below.

While work on the Sin Temple was in progress at Khafaje, a great effort was being made at Tell Asmar and elsewhere to establish the chronology

of the Early Dynastic period of Sumerian history, which had already been subdivided into three recognisable sub-phases. All three of these phases—"E.D.I., II and III" were represented in the history of the Sin Temple, as well as two earlier periods, at that time known as "Proto-Dynastic". Any archaeological evidence, therefore, which could be found to throw light on the actual duration in years of the various building periods had to be considered as of first-rate importance. With this in mind, Delougaz was able to exploit his newly perfected digging technique to make some complicated and very brilliant deductions.

One astonishing accomplishment in this respect is illustrated by two photographs in the report. The first shows a stairway of mud brick, approaching the north-east entrance to Temple 7. Included in it are two distinct occupations of the building, which are marked "a" and "b". The floor in the foreground and the three bottom steps belong to the first occupation. The floor of the second occupation has been cut away, showing the corresponding wall-face, slightly over-hanging that of the first. Delougaz[1] recognised that this overhang was caused by successive replasterings of the walls during the second occupation. He had already observed locally that, in any important mud brick buildings in use at the present day, the plaster deteriorates rapidly and the walls are therefore conscientiously replastered each year, usually in the autumn before the winter rains. He accordingly realised that, if the number of replasterings could be counted, a corresponding number of years could probably be attributed to the occupation period concerned. This, as can be seen in the second picture, he was actually able to do, and approximately sixteen years could be estimated as the duration of occupation No. 2 in the seventh Sin Temple. Having seen that these sixteen years corresponded to a rise of seventy-five centimetres in the floor level, he was able to correlate this with the total accumulation of debris in each of the temples: and since these, by the character of the objects found in them, could already be attributed to the successive phases of the Early Dynastic period, it became possible, allowing a fairly wide margin of error, to estimate the length of the Early Dynastic period for the first time on archaeological evidence. The ingenuity and exemplary care with which this experiment was conducted may, it is hoped, in itself serve to refute any general imputation of ineptitude in the "methodism" of Near Eastern excavators.

During the years that this work was in progress at Khafaje I myself had been working with another gang of Sherqati workmen at the headquarters

(1) *op.cit.* p. 125ff.

site, Tell Asmar. Like Delougaz, I had found a small Sumerian temple and traced back its architecture through all three phases of the Early Dynastic period to an original, very small chapel, about contemporary with Sin I. One of these temples had three separate shrines; and it was beneath the pavement of one that I found the cache of twenty-one Sumerian statues, including the so-called "cult-statues" of the God Abu and his consort, which have today become familiar in books on Sumerian sculpture. A photograph (PL. 17) taken while this operation was in progress has some technical interest, because it was taken with a camera suspended from an ordinary kite. I had at the time recently visited the Oriental Institute excavations at Megiddo in Palestine, and watched the process of taking air photographs from a stationary kite-balloon. But it seemed to me, both that the apparatus involved in this experiment must be extremely expensive and that a lot of unnecessary time was wasted on the operation. My own attempt to simplify the process was surprisingly successful. I used two six-foot naval kites, flying in tandem, and suspended beneath them a cheap camera with an automatic release and swivels for retaining it in a vertical position. Admittedly this was no more than a rough and ready way of getting low air-verticals; but out of some scores of pictures which were taken in this way, a dozen or so proved extremely revealing and useful. One could, for instance, recover quite large sections of the ancient town-plan, by photographing the unexcavated surface of the mound after rain; for the tops of the walls were found to dry and change colour much more rapidly than the filling in between. (PL. 18)

But, while still engaged in recording the Abu Temple at Tell Asmar, I had at the same time become involved in what proved to be a much more frustrating operation. Eshnunna, which is the ancient name of Tell Asmar, had been an important city-state during the Isin-Larsa dynasty at the beginning of the second millennium B.C.; and we were also excavating a complex of public buildings belonging to that period, known by the name of its original founder, Gimil-Sin. Here, as so often happens in Mesopotamia, the chronology of the stratified remains presented very little difficulty, because the buildings at successive structural periods were constructed partly of kiln-baked bricks stamped with a pictograph inscription bearing the name of the prince who had rebuilt it. Not only was his name given, but very often also that of his father and son; so that a genealogical table was comparatively easy to establish. But another element in these texts proved more puzzling. It was the repeated references to another and evidently much larger temple dedicated to Tishpak, the patron god of

Eshnunna. This seemed, (like the Marduk Temple at Babylon, for instance), to have been the most important building in the city. For the last three years of the excavation at Tell Asmar, I had been using a substantial part of my labour in a fruitless attempt to locate it by a system of search-trenches all over the site. (PL. 19) But a bi-product of this rather unproductive digging was the gradual perfection of wall-tracing technique among the Sherqati workmen: and this at a certain moment produced one of those phenomena of mud brick archaeology, which would hardly be conceivable in any other setting.

Digging one day to the north of the Gimil-Sin Temple and nearer to the main summit of the mound, we began to obtain the outline of a building with walls from fifteen to eighteen feet thick, standing some six or seven feet high. There was of course great excitement and a further gang of men was brought from elsewhere to speed the recovery of the plan. After a few hours work, it had been extended to cover almost the whole southern flank of this main hill. But, although the actual shape of the building seemed to be emerging, I was continually puzzled by the fact that, in following the face of a wall, a tracer would repeatedly be found to have turned three right angle corners in the same direction and ended up where he started, having merely articulated what appeared to be an isolated block of masonry. A series of these now stood up out of the ground in regular formation, making no architectural sense whatsoever. I then began carefully to test the material of which they were made: and I found that they consisted entirely of stratified occupational debris with some quite large pottery vessels in it. Long thought and further minute investigation finally brought an explanation. What my men had been tracing were *not* walls at all, but the foundation trenches carefully and completely dug for a temple or palace which had never in the end been constructed. The upstanding masses of masonry were merely the "dumplings" left between the trenches. The trenches themselves had afterwards again become filled with debris, which we had removed, thereby resurrecting the ghost of an abandoned building project. The Tishpak Temple was unfortunately never discovered.

One more of these Sumerian mounds in the Diyala region must be mentioned; Tell Agrab, about fifteen miles to the south-east of Tell Asmar, which I excavated myself as a subsidiary undertaking in 1936-37.[1] For at the very outset this site provided another remarkable illustration of how our technical understanding of mound anatomy had improved during the preceding five years.

([1]) *op.cit.* Ch. III.

Tell Agrab is really a complex of mounds—not very high, but curiously impressive because of its isolated situation in an otherwise flat alluvial desert, with a curtain of snow mountains in the remote background. Its existence had long been known to us owing to the fact that, on a very clear day, it could be seen from our roof-top at Tell Asmar; and once, under unusual weather conditions, its silhouette appeared in a mirage,—upside-down—a little above the horizon. In our own minds too, its archaeological possibilities retained the character of a mirage, until 1936 when circumstances suddenly made its excavation a practical possibility.

After a preliminary visit to the mound with a Bedouin guide, the first practical step seemed to be to make the place more easily accessible by car. Avoiding such hazards as ancient irrigation canals, it proved possible to mark a circuitous track, keeping to the dead flat surface of the *chôl*, so that the maximum speed of the vehicle could be retained throughout the whole distance. In this way the fifteen miles journey could, on a dry day, be covered in an equal number of minutes. During the five days which I spent in making a topographical survey of Tell Agrab, it became clear that in these circumstances living quarters would not be necessary at the site itself.

For the excavator in person to make the initial survey of a mound of this sort has obvious advantages. One feature which I had particularly noticed on the north-east side, within the line of what was evidently an enclosing city-wall, was a striking amphitheatre-shaped area, more or less free of surface pottery and surrounded on three sides by higher hills. One had learnt by experience that in a Sumerian city, if an important temple was ever allowed to fall into ruins and for some reason never rebuilt, later inhabitants of the site would continue indefinitely to consider the site as sacred and refrain from building any profane structure on top of it. This could tentatively be accepted as the explanation of the "amphitheatre", especially as the rough hills around it were covered with surface pottery which enabled one to identify the remains beneath as those of post-Sumerian-Akkadian private dwellinghouses. Careful examination of the amphitheatre itself also revealed the existence of some very extensive mud brick structure directly beneath the surface. So, on the last day of my survey, I brought over with me from Tell Asmar my best wall-tracer, Saleh Hussein, and I put him to scrape the surface at this point. Ten minutes later he came running across to where I was still surveying with a large fragment of limestone relief in his hand, of the sort used for ornamenting the walls of Early Dynastic temples. In this case therefore, no

doubt remained as to where it would be desirable to start digging: and in fact, three months later there had emerged on this site the ground-plan of the largest Sumerian building yet discovered; and our antiquities magazine at Tell Asmar was piled high with beautiful objects we had found among its ruins. (PL. 20) The exact spot where I had put Saleh Hussein to scrape on that first day, was on top of the high altar in the main sanctuary of what is now known as the "Shara Temple". (PL. 6)

At Tell Agrab I used fifty Arab workmen, twenty of whom were our invaluable Sherqati experts. A photographer and an epigraphist visited me every day that I thought necessary, and all the recording and treatment of antiquities was done in the lavishly equipped expedition house at Tell Asmar. On the actual excavation I was often entirely alone, moving from one skilled workman to another, whenever they got into difficulties or finished a particular task. By many today this would be regarded as a preposterously under-staffed operation. But I do not myself believe that the field-notes and catalogues from which I produced the final publication lacked anything in method or detail; and I am convinced that I did not miss any stratigraphical evidence. Over part of the site, I excavated two previous building-levels. But out of the five hundred or so objects which were registered, I do not remember one of which the provenance remained equivocal or whose find-spot could not be exactly placed, both in the plan and in one of the many sections. It should be added, of course, that this was clearly not a model excavation. The situation was due to *force majeur*. Frankfort, from his headquarters at Tell Asmar, was by this time remotely controlling excavations simultaneously at Tell Asmar itself, Khafaje and at Ischali (where Thorkild Jacobsen was recording a temple-palace of the Isin-Larsa period); so that the staff of fifteen Europeans and Americans was very fully occupied. But illicit digging was beginning on a big scale at Tell Agrab; and, rather than have it reduced to the state in which we had found Khafaje, it was necessary to take immediate action.

On this occasion, as on many others, much credit for the results went to the skilled Arab workmen. They were living under very primitive conditions, in holes in the ground covered by reed matting, and they were dependent on motor transport from Tell Asmar to bring their bread and vegetables every day. If, as sometimes occurred, heavy rain fell for several hours on end, the entire desert, as far as the eye could see, became a shallow lake, from which the mound emerged looking like some uncanny island: and the ancient irrigation canals would run deep in water. Sometimes this would make transport impossible for several days, and they

would be compelled to return on foot to the base-camp. It is easy to imagine how these periodical floodings and other difficulties interfered with the continuity of one's excavations. And they may serve to remind one how rarely ideal conditions are obtainable for an excavation of this sort. It goes without saying that a capacity for improvisation is an almost indispensable asset to any Near Eastern digger.

CHAPTER III

South Iraq: Sumerian Sites

The excavations described in the previous chapter must be considered merely as the contribution of a single expedition to the recovery of Sumerian dynastic history. During the latter part of the interim between wars, there were of course others whose work can under no circumstances be ignored. Among these were, for instance, the patient and assiduous operations of Nöldeke's German expedition at Warka, the Erech of the Old Testament.[1] Here, too, were temples worthy of a major city-state, and they found a great "temenos" precinct, laid out on a monumental scale. The plans of these buildings, continually rebuilt during the dynastic and later periods, were laboriously disentangled from an acre or so of mud brick, meticulously cleaned and articulated. In the field records which appeared in periodical reports, one sees how every individual brick is not only given its correct place, but the surviving height of the structure to which it belongs is indicated by cast shadows superimposed in pen and ink; and one wonders sometimes if the excavators' meritorious preoccupation with the minutiae of architectural remains may not have tended to obscure the broader conclusions to be drawn from them. But here too was a total reversal of nineteenth century digging methods, investing field archaeology with the distinction of a vocational discipline. Nor could any review of Mesopotamian research during these years progress far without listing the prodigious contributions of the British Museum excavations at Ur. With these we shall now find a natural connection in the sequence of personal experiences on which this narrative hangs. But before this continues, having once referred to a British expedition led by one of the greatest excavators of the present century, the moment has perhaps arrived when a considerable digression must be allowed on the theme—"Let us now praise famous men."

It is difficult to write of the late Sir Leonard Woolley without becoming conscious of him as a living personality. Physically he was a small man. In

(1) The bibliography of these excavations is a lengthy one. The principal reports have been published since 1928 by J. Jordan, H. Lenzen and others in the series *Uruk Vorläufiger Berichte*, of which No. 17 is now in preparation.

photographs of his last expedition at Atcharna when he was nearly seventy, hatless and still wearing khaki shorts, he could almost be mistaken for a prematurely ageing schoolboy. But his appearance only partially concealed a dynamic and attractive personality, which can never be quite forgotten by those who knew him well. Where even his attainments are concerned, it is difficult to accommodate oneself to the obituary past tense. They have perhaps best been summarised by M.E.L. Mallowan, who served for so long as Woolley's trusted assistant at Ur. Of the three qualities which contributed most to his distinction as an archaeologist, Mallowan draws attention first to his consciousness of how much a knowledge of architectural development can add to our understanding of ancient society. Next on the list he mentions Woolley's incomparable sense of craftsmanship and appreciation of the motive behind it. Wherever he found a thing produced by human hands, he tried to visualise the process by which it was created and to share the aspirations of its maker. And lastly, Mallowan says, came his great gift as a writer and the fluent style which entranced a multitude of readers all over the world. He adds that good writing, which can only be achieved by good reading and clear thinking, should be the aspiration of all archaeologists.

Certainly Woolley's own excavation reports are among the few which could be regarded as a criterion in this respect. And undoubtedly it is as a result of this last accomplishment that Woolley's name has come to be so widely known. Under the first and second headings which Mallowan mentioned, far less has ever been said, though Woolley was in fact himself a magnificent craftsman and had received a well-deserved honorary degree in architecture. And it is these other attainments which now particularly concern us, if we are to recall the success of his many excavations. Readers interested in archaeology all over the world know that, in the Royal Tombs at Ur, Woolley found a treasure almost as rich and splendid as that of Tut-ankh-amen's tomb in Egypt. Students without number have seen pictures of the great "death-pits" with their retinues of ornamentally dressed attendants and the precious objects buried in the tombs themselves. But how many outside the archaeological profession know that the Royal Cemetery alone revealed more than two thousand ordinary graves and that the contents and particulars of every one of these were scrupulously recorded and afterwards published by Woolley himself? And how many know that the royal graves were actually discovered during his very first season's work; but that Woolley did not feel technically competent to excavate them until four years later. In his excavation catalogue, the

first hoard of objects from one of the "death-pits" is dated 1922 and the entry is significantly headed—"Jewellery in gold, lapis-lazuli and carnelian; seventh century B.C."[1], the date of course being approximately two thousand years too late. Having once seen this material, Woolley sealed off that part of his excavation and temporarily transferred his activities elsewhere. He realised that his workmen were still ignorant, reckless and dishonest. And besides, as he said himself, "The archaeology of Mesopotamia was then in its infancy and there was no means of dating the small objects that came out of the graves. Our object was to get history, not to fill museum cases with miscellaneous curios, and history could not be got until both we and our men were duly trained."[2] It was four years before Woolley felt that his proficiency in the technique of Mesopotamian excavation and the training of his men justified a new approach to the great cemetery.

Furthermore, this fine example of professional restraint was shown by Woolley in the face of an everpresent need for publicity. For he worked at a period when the acquisition of funds for excavating depended very largely on the munificence of private individuals and the interest of wealthy newspapers. Under these circumstances it was essential for him to publicise his finds and his lively imagination invested him with a natural faculty for publicity. And so, through the pages of the daily press came the exciting and stimulating story of how, in his deep sounding beneath the Sumerian cemetery at Ur he had found traces of the Biblical Flood and of a race of people living before the Flood. And it was only his archaeological colleagues, (perhaps a shade less interested in the Biblical interpretation), who were able to understand the technical perfection of the long section which he cut and recorded along the high cliff-face of his sounding at the south-west end of his cemetery excavation and the chronological sequence which it revealed.

Then again, behind this and other soundings which Woolley made at Ur, there was a clearly conceived and logical purpose, which had suggested itself to his active mind at the very outset of his work. Soon after his work at Ur started, he had collaborated with H. R. Hall in the excavation of the little mound called Al'Ubaid, which is just visible from the summit of the Ur *ziggurat*. Between them they exposed the brick platform which had supported a Sumerian temple;[3] and they recovered from it the magnificent architectural ornaments, some of which are to be seen in the British

(1) Quoted by M. E. L. Mallowan in "Iraq" Vol. XXII 1960. p. 10.
(2) Mallowan, *loc.cit.* p. 10.
(3) Mallowan, *loc.cit.* p. 16.

Museum. But while this work was in progress, Woolley also cut a trench into the remains of a prehistoric settlement which spread out eastward from the foot of the temple mound. He found the painted pottery and reed huts of a pre-Sumerian people; and since there seemed to be nothing but clean soil for three or four feet beneath them, he concluded that these early settlers had occupied a natural island in the drying marshes of Mesopotamia. In the professional controversies that continued for many years after this discovery was made, it came familiarly to be referred to as "operation huts-in-the-marshes": and it will presently be possible to see how, on this occasion, a too-impulsive conclusion temporarily distorted the historical picture which he was attempting to reconstruct. But for the moment Woolley was quite rightly obsessed by the necessity for dating the epoch to which these marsh-dwellers belonged and giving them their correct place in a properly authenticated stratigraphical sequence. This was the main purpose of his soundings at Ur itself.

They were entirely successful. Supplemented by evidence from prehistoric soundings at other sites such as Jemdet Nasr and Warka, they established a complete sequence of prehistoric cultures, of which for the moment the "Al'Ubaid culture" was the earliest. And if, in his so-called "Flood Pit", identical traces of the "huts-in-the-marshes" appeared above *and* below what was called the "Flood deposit", these terms were only meant for publicity purposes: and for archaeologists his section already envisaged an interesting sub-division of his Al'Ubaid period into distinct phases.

So here were the two sides of Woolley's work: an appeal to the public by means of interpretation and presentation of his results (with undoubted educational advantages in addition to their practical purpose of obtaining funds); and behind this the patient and meticulous work of a research scholar. The results of twelve seasons of arduous digging at Ur alone can be seen in his publications. First, annually came the admirable preliminary reports, written often on board ship on the way home, when the whole fabric of his discoveries was still fresh in his mind. And then, in the years before his death, sixteen heavy volumes of final publication—as Mallowan has called them—"a mine of information, a deep repository which will grow richer as the years pass and the common store of knowledge is continually pooled."

But now, to examine Woolley's place in the new forum of archaeological methodism. By all the most recently devised codes of procedure and disciplinary generalisations, Woolley was an unconventional excavator,

to say the least of it. To begin with, during the whole of his twelve campaigns at Ur, he never employed more than five assistants—an astonishingly small number considering that they had to control the activities of a labour gang consisting of from two hundred to two hundred and fifty men, and that during that time more than twenty thousand small but valuable objects were in the process of being found. One partial extenuation of this idiosyncrasy was that he tended to depend largely and increasingly on his Arab staff. A great deal of practical responsibility was taken off his shoulders by his foreman, Sheikh Hamoudi Ibrahim. Hamoudi was a Syrian of strong character and remarkable talents, which afterwards acquired for him the position of Deputy in the Syrian Mejlis. Woolley had acquired his services before the First War, when he was excavating Carchemish on the Turco-Syrian frontier, and Hamoudi had at one time saved the life of T. E. Lawrence who was also a member of the excavating party. By the time Woolley came to excavate the Royal Cemetery at Ur, Hamoudi had been joined by his three sons, Jahya, Ibrahim and Alawi, all of whom were brilliant archaeological craftsmen, and the first of whom, Jahya, completely took charge of Woolley's photography. But it was of course the Director himself whose inexhaustible energy and multifarious talents animated the whole undertaking. The fact is that Woolley, like Flinders Petrie and others, belonged to a generation of archaeologists whose individual genius kept them withdrawn from any confidential relationship with their staff. To his assistants he deputed specific work and himself inspired sufficient loyalty to ensure that it was carried out to the limits of their ability. But the purpose and progress of his operations and the sequence of conclusions to which it led, were seldom discussed with them. It was in fact not unusual, after an excavation was over, for his junior assistants to read with interest in the newspapers the details of discoveries in which they themselves had presumably taken part. This was the case, for instance, with the attribution of certain rifled tombs to the Second Dynasty kings of Ur, on whose identity Woolley had apparently remained undecided until the season's digging was finished. In these days, when the secondary function of a field director is to train younger archaeologists, this form of reticence would have the most obvious disadvantages. Also, it is hardly surprising that his conclusions were occasionally wrong. To recollect that his dating of the Royal Tombs now proves to have been approximately five hundred years too early is perhaps to be "wise after the event", since his interpretation of the available evidence has been corrected by more recent discoveries. But undoubtedly certain theories which he

devised, mainly for purposes of publicity, required the most tortuous arguments to justify them. One remembers for instance, how, in his soundings, clay deposits which appeared out of context in relation to the Flood, became "quays for shipping." And soon, in the press Ur became the "Venice of the Ancient East."

But here again, on the serious scientific side, nothing could detract from the almost intuitive logic which distinguished Woolley's deductive reasoning. As Sir Mortimer Wheeler has understandingly observed quite recently, "The confident but always acutely experimental intelligence underlying the remarkable discoveries which again and again advertised his achievements was too often of a kind that escaped the easy comprehension of his cloistered critics. In this unimportant sense he suffered from success."[1] In the polemical phraseology which has sometimes been used to point a contrast between the shortcomings of old-fashioned archaeology and the newly perfected academic discipline, Woolley's work has tended to be included by implication in the general target for categorical disparagement. Posthumous testimonials like that quoted above must be treated as a welcome corrective.

Woolley's Ur excavations came to an end in 1934: but three years later some new light was thrown on the problems which he had encountered at Al'Ubaid by a further short investigation of the site, in which I myself took part. We have spoken in an earlier chapter about Delougaz' excavations of the so-called Oval Temple at Khafaje and regretted that only its plan could be recovered, owing to the denuded state of its foundations. The central problem regarding the character of the temple itself remained unsolved. The oval *temenos*, with its double line of outer walls, enclosed an open courtyard; and in the centre of the courtyard, a rectangular brick platform was found with a stairway leading up to it. In his search for some clue to the reconstruction of such a building, Delougaz was reminded that almost the only parallel was to be found in the similar, though much better preserved temple platform of the Early Dynastic period, found ten years earlier by Hall and Woolley at Al'Ubaid. Here, though once more the temple itself had not survived, something was to be learnt about its architectural character from the rich facade ornaments which, as we have already mentioned, Woolley found stacked against the base of the platform. But, in excavating and removing these, he had covered much of the surrounding area with dumped earth and no further investigation had been made of the platform's immediate surroundings. Delougaz now developed

(1) Sir M. Wheeler in "Antiquity" No. 142. June 1962. p. 145.

a conviction that further significant parallels between the two temples could be established. So in 1937, with Woolley's willing agreement, he and I returned to the site for ten days to make a further short examination.[1]

Our discoveries during this small "posthumous" excavation at Al'Ubaid could almost be described as dramatic and were certainly most satisfying to Delougaz. He and I had taken with us our most competent wall-tracers from the Oriental Institute excavations. These two men were first employed in cleaning the brickwork on the summit of the platform, in the hope that some vestigial remains might survive of the temple foundations. They did indeed discover, at the head of the stairway, brickwork of two different types, as though some building had been more than once reconstructed. But unhappily the denudation caused by the weather had completely defaced the shapes of the walls. Next therefore, we transferred them to the pavement at the foot of the platform on which the great bronze lintel, copper bulls' heads and other architectural ornaments had been found by Woolley and Hall, and which was still easily recognisable. Moving outwards from the platform between Woolley's dumps, we scraped and examined the ground with extreme care; and soon, as we had half expected, we were able to identify both faces of a wall several metres thick. The two men were then set to trace this wall in opposite directions; and they proceeded at such a pace that hardly an hour elapsed before the evidence we were expecting began to appear. The wall was in fact developing in a circular direction, and, as it gradually straightened itself out, it became clear that the Al'Ubaid temple, like its counterpart at Khafaje, stood in the centre of an oval enclosure. In the days which followed the men completed the circuit of the wall, and, during the last few hours before we were due to return by train to London, they met and were able to outline the plan of an entrance gateway on the south. (PL. 21) The oval enclosure at Khafaje was not, then, a freak of architecture due to some regional idiosyncrasy of the Diyala people, but a common practice in building Sumerian temples of the dynastic period. One might go further and say that any reconstruction of the raised temple at Al'Ubaid (such as has recently been made in the British Museum on the basis of the facade ornaments), could be supposed to present an equally probable picture of the temple crowning the platform of the oval at Khafaje.

But, during our ten days at Al'Ubaid, another discovery had been made which enabled us to draw inferences about the site such as would hardly have been possible for Woolley in the state of his knowledge when he ex-

(1) Published in "Iraq" Vol. V. Pt. I. 1938. p. 1.

cavated it. Among the debris around the temple platform we had picked up tell-tale indications of a much earlier temple on the same site—cone-shaped terracotta mosaics and very small rectangular baked bricks with circular holes into which ornamental rosettes could be fitted. From our knowledge of the German excavations at Warka, we could conclude from this that a temple had already existed here in the rather awkwardly named "proto-literate" period before the Sumerian dynasties began. This was again confirmed in a rather curious way. At some time in the history of the mound, an earlier excavator had cut a trench diagonally into the corner of the platform, probably in search of a foundation deposit. When we emptied this trench of the sand which had drifted into it, deep under the dynastic platform we found a line of foundations in red sandstone, running at an angle of forty-five degrees to the platform itself. Here then was an earlier version of the temple in proto-literate times, and there was no reason to doubt that this too was merely a rebuilding of an original shrine contemporary with the settlement on the adjoining hill—that is, of the period now known as Al'Ubaid in about 3500 B.C.

This small exploit at Al'Ubaid marked the end of our work for the Chicago Oriental Institute; and I was myself not able to pursue the same line of investigation further until some years later, when I returned to Iraq as Adviser to the Arab Directorate of Antiquities. A most satisfactory sequel was then provided by the excavation of a site called Tell 'Uqair which I undertook with Arab colleagues during the first year of the War.[1]

'Uqair was again a very small mound, some hundred miles north-east of Ur and about equidistant between the Tigris and Euphrates. Once more I was led to it by the discovery there of the tell-tale mosaic-cones; and I found it to be in general shape and lay-out almost uncannily similar to Al'Ubaid itself. There was a low settlement hill, its surface littered with the now-famous Al'Ubaid pottery, and beside it a smaller but higher mound, from which the cone mosaics had been collected. Furthermore this smaller hill appeared to me to consist almost entirely of solid mud brick, as one would expect if it represented the remains of a temple platform. (PL. 22)

When I recollect our approach to the excavation of this mound, I cannot help being incidentally reminded of a passage italicised in one standard work on modern archaeological method. It reads—"There is no method proper to the excavation of a British site which is not applicable—nay, must be applied—to a site in Africa or Asia."[2] For this is a contention

(1) Published in the "Journal of Near Eastern Studies", Vol. II 1943

(2) Sir M. Wheeler. *Archaeology from the Earth.* Oxford 1954. p. 22.

which the circumstances at this particular "Asiatic" site could be taken to disprove by a simple process of *reductio ad absurdum*. To have divided the temple hill at 'Uqair into a system of rectangular soundings divided by standard "balks", and to have recorded a section in the face of each, would have occupied the whole of our first season's excavating and probably in the end resulted in a confused tangle of negative conclusions. What we actually did, after making a topographical survey of the site and covering it with a grid of pegged squares, was, first, meticulously to scrape clean the entire surface of the hill. In this case there was actually no further need to articulate the brickwork which appeared, since the distinctive colour of the bricks and mortar joints was sufficient to enable us to outline the structure beneath, before even starting to excavate. The result seemed at the time almost too good to be true. Here indeed was a temple platform: but the walls which we found ourselves tracing at the summit of the mound were not the platform itself but those of the temple which stood upon it, and this in places clearly remained standing to a height of over six feet. Furthermore, having already outlined its plan on the surface, we were able in tracing the walls, to enter the building in an orderly manner through the main entrance door. And, as we reached the inside of the first vestibule, to our complete astonishment we discovered that the inner wall-faces were covered with painted frescoes. Work on the other side of the hill meanwhile had revealed the source of the mosaic cones, which formed an ornamental band along the parapet of the platform. So the temple with which we were dealing was not, as at Al'Ubaid, of the Sumerian Dynastic period, but of the earlier and little known proto-literate, corresponding to the stone foundations which I had discovered under the later platform at Al'Ubaid itself. (PL. 23)

As for the wall-paintings, one began to see, first of all a dado of plum-coloured paint, exactly matching that used in the proto-literate painted pottery of Jemdet Nasr; then a band of elaborate geometric ornament, and above this the feet and legs of men and animals, evidently forming part of a mythical scene of the sort one sees in cylinder-seals of the period. But now for ourselves came a period of great anxiety. For we found that what our wall-tracers were cutting into, was not the ordinary soft "fill" inside a room, but very hard and carefully laid mud brick. Now we could understand why the part of the building which had survived was so remarkably well-preserved. The whole of it had at some time been filled up solid with brickwork, converting it into an upper storey for the platform, upon which a yet higher temple could be built. This was desperately serious,

since the wall-paintings were found to adhere more strongly to the filling than to the plaster on which they were painted; and when this filling was removed they came away with it. Now was the time at which, under different circumstances one would have closed down the excavation and awaited the advice of European experts. But this was the first year of the war and no such help could possibly be obtained. In the end we did all that was humanly possible. Infinite time and patience were necessary, first to reduce the filling laid against the face of the walls to a mere skin; then to remove what remained with such implements as razor blades and porcupine-quills, so that the paint could be fixed and preserved for recording. What emerged, is today fairly well known; particularly the altar frescoes with their spotted guardian lions on either side and the altar front painted like a little miniature temple. But the upper parts of the men and animals in the processional scenes were unfortunately lost forever.

So here at 'Uqair was a situation exactly resembling that at Al'Ubaid itself; a settlement of the "'Ubaid" period on the larger hill and beside it a temple which had been repeatedly rebuilt in later times. Our greatest curiosity was to know what, at both sites, would have been the character of the original Al'Ubaid-period temple, contemporary with the settlement. For Sir Leonard Woolley had familiarised us with the idea that the makers of the Al'Ubaid pottery were a primitive marsh-dwelling people who built only in wattle-and-daub. It was at Tell Abu Shahrein, the ancient city of Eridu, that I was later able to solve this problem; and it is to that site, perhaps the most extraordinary from a technical point of view ever excavated, that we must now turn.[1]

From the beginning Eridu seemed a likely source of information on such subjects, if only because of its historical reputation for extreme antiquity. The Babylonian "Legend of Creation" in the "bi-lingual" version simply states—"All the lands were sea: then Eridu was made." There is much else in Sumerian literature to confirm the traditional priority of the city, since it was the home of Enki, God of the Abyss, who was also the source of human wisdom. Here he dwelt in his shrine upon the shores of the "deep" which, like the so-called "waters" in Genesis, was divided as a preliminary to Creation. The site as one sees it today certainly seems a perfect setting for such a legend. The mound stands about fourteen miles south of Ur, in a totally isolated wilderness of dust and camel-thorn. It takes the form of a platform, about three hundred yards square, with the ruins of a *ziggurat* tower rising above it at one end. (PL. 24)

(1) Published in "Sumer" Vol. III 1947, Vol. IV 1948 and Vol. VI 1950.

If this description of one of the most striking-looking mounds in Mesopotamia sounds a little prosaic, let us see how it emerges from the more picturesque idiom of a past generation. The late Dr. Campbell-Thompson, who excavated there in the final year of the First World War, recorded his impressions of it as follows.[1] "The general appearance from the desert is of a flat, low mass of brown earth, with steep sides rising to nearly forty feet, the crest appearing to maintain an almost unbroken level, except that at the north are the remains of a lofty *ziggurat*, now whittled down by the rains to a sharp peak, piercing the sky like a dagger.... But on approaching the mound, one sees the rough lumps of limestone and sandstone which still buttress its sides, scattered about in disorder. The mound itself has practically become a basin; the interior, having for the most part consisted of buildings of unbaked brick, has melted into formless clay under the torrential rains which pour down during the winter. The temple tower, playing the part of a small mountain at the upper end, has conducted the rushing water down its slopes, which have easily gnawed channels both in the mass of disintegrating brickwork of the houses on the mound, and in the windborne sand which settles in every cranny and near every wall.... As one looks down from the *ziggurat*, the whole of the ruins which lie to the south-east at one's feet consist of water-borne hummocks, ravines, drift-banks of sand against some ancient wall, and, after rain, pools of water. From the *ziggurat*, as far as the eye can see there is naught but awful solitude. You look down on sombre desert which encircles you for miles. Northward lie the flat lands, yellow in April, and unrelieved except for sparse arabesques of salt, spreading like mare's tails in a breezy sky...." (PL. 24)

It is in a way curious that Campbell-Thompson lays so much emphasis on sand. For it is in fact dust rather than sand which makes the most conspicuous feature of the whole setting, with the exception perhaps of the wind, which for days on end blows it in suffocating clouds across the landscape, leaving a rippling dune spreading far out into the desert on the leeward side of the mound. Before starting to excavate, we built ourselves fairly substantial quarters, bringing Sumerian bricks from the ruins of Woolley's expedition house at Ur. But when we returned to it for a second season's excavating, it was buried to the eaves in dust and had itself to be excavated before we could re-occupy it. Many days had also to be spent in emptying our trenches of dust before we could continue the work.

But to return to the actual anatomy of the mound, my impression

(1) "Archaeologia" Vol. LXX 1920.

from the beginning, judging from past experience, was that what we were dealing with was once more simply a temple platform, like those which have already been described, but on a much larger scale. Upon this the *ziggurat* and perhaps other temples had been built by the kings of Ur. (And indeed, as we soon learnt from the inscribed bricks, they had even been rebuilt by the Babylonian, Nebuchadnezzar as late as the sixth century B.C.). The supposition in this case would be that the actual fabric of the platform must consist in part of the filled-up remains of earlier buildings, just as the temple at 'Uqair had been filled to make a second storey for the existing platform. This proved to be substantially the case. Campbell-Thompson had complained that wherever he dug into the platform he encountered nothing but loose sand. We ourselves at first found very much the same thing: but, unlike Campbell-Thompson, we were equipped with a small-gauge railway and could continue to remove the sand until we reached the mud brick walls which were as it were supporting it. We were then able to expose part of a complex of small public buildings, still standing in some cases as much as eight feet high, so that their doorways and even windows had survived. Such pottery as remained in the rooms, showed them to belong to the Early Uruk phase, just before the Proto-literate period. At this time they must have been subsidiary buildings in the *temenos* surrounding the temple. They and probably the whole city of Eridu must, at some time around 3400 B.C., have been temporarily abandoned and left unoccupied; so that the roofs fell in and the buildings gradually became filled up to their eaves with drifting sand. When the place came to be reoccupied in the proto-literate period, it was easy for the new builders simply to surround the whole complex with a retaining-wall and seal in the sand-filled buildings with a rough pavement of bricks, thus providing a magnificently spacious emplacement for their new temple.

So obviously, our next duty was to begin a systematic search for the prehistoric temple itself. We already had some idea of what a magnificent building it must be, since the surface of the mound was everywhere littered with fragments of architectural ornament which had once decorated its facades. We have already mentioned, for instance in connection with the smaller temple at Tell'Uqair, the mosaic cones which were made of baked clay with their ends dipped in various-coloured paints. But here at Eridu, the cones were of coloured stone, every one laboriously chipped into shape and polished at the end. These were the normal sized cones—about six to eight inches long. But there were also others up to a foot long, cast in gypsum; and these had their projecting ends sheathed in what must

have been polished copper. There were also other kinds of inlay ornament as rich as the much later decoration of the Al'Ubaid temple.

Logically, the place to look for the temple, we thought, would be at the north-western end of the platform where the later *ziggurat* stood. Even Campbell-Thompson's report encouraged one in this view. For, of a trench which he dug in this area he said—"So much puddled clay, the remains of dissolved mud brick, appeared just beneath the surface in this trench—that it was early abandoned." And sure enough, examining the surface at the base of the *ziggurat* ruins, we found the usual promising area of solid mud brickwork. But in this case it could be seen to extend as much as thirty yards on either side of the *ziggurat*, and two weeks had to be spent in articulating every brick over this entire area. The result was extra-ordinarily puzzling. (PL. 25) Projecting from beneath the southern corner of the *ziggurat* ruins, one could recognise the plan of a rather unexpectedly small building of the temple type. But this was surrounded by no less than five concentric rectangles of brickwork, each constructed of a different type and size of brick, while the outermost casing was faced with a sloping revetment of stone. Much thought and discussion were necessary before we came to understand that each of these casings represented an extension and enlargement of the brick emplacement on which successive predynastic temples had stood. All had been levelled to the ground when the foundations of the *ziggurat* were laid including the last and most pretentious of all; a building whose platform was faced with stone and, as we now discovered, ornamented with panels of burnished bronze mosaic.

This was of course a great disappointment. But now, at least we could turn with heightened curiosity to the small temple whose ruins had survived, nested as it were in the centre of the encircling later platforms. And our first and most satisfactory discovery was that it contained rich deposits of pottery—not the dull monochrome of the Proto-literate or Uruk periods, but the painted ceramic of Al'Ubaid itself. Here then was the answer to our speculations about South Iraq's earliest settlers; not a primitive shrine of wattle-and-daub but a well-built temple, complete in every respect, with central sanctuary, altar, offering-table and panelled facades outside. Here and there, however, were details to remind us that we were back in the swamp world of the drying Mesopotamian delta. The principal form of offering made in this temple (presumably dedicated to Enki), was *fish*. The whole area around the rectangular offering-table was cluttered with fish-bones and a store-room beside it was piled with a thick, rather glutinous deposit, where surplus fish offerings had apparently been dis-

posed of by burning. One very complete skeleton was afterwards identified as a species of sea-perch, which inhabits the brackish water of estuaries. It was strange, in this connection, to remind oneself that Eridu is now a hundred and fifty miles from the sea.

This building came to be called Temple VI, because of the five later platforms which clustered round it. It was not of course the earliest foundation of Enki's shrine. After clearing and recording it we found another similar building beneath; and soon we were involved in the familiar task of examining one temple after another, right down to the deepest levels of the city's earliest remains. Temple XVI was the earliest building recognisable as a religious shrine and certainly the earliest yet found in Mesopotamia. It was a tiny building; no more than five yards square, with a single doorway, altar and offering-table. But it was already built of sun-dried brick. Beneath it was clean sand, after penetrating into which for another fifteen feet, the danger of a collapse in the walls of the sounding became so great that it had finally to be abandoned. I do not personally think that, if we had been able to continue, we should again have found traces of human habitation beneath. For it would be logical for these settlements to have been built on the first sand-dunes to appear in the marshes.

In any case, we had learnt much from the sounding. For the painted pottery in the temples had passed through three successive phases. First, in Temples VI and VII there was normal Al'Ubaid pottery such as was found in the settlement at Al'Ubaid itself and at 'Uqair. Secondly, in Temples VIII-XIII, there was more elaborately painted ware, which has parallels at Tepe Gawra in north Iraq. And thirdly, in Temples XIV-XVI we found a quite different and earlier pottery, called by the Germans after the village where it was first found by them, Qal'at-Hajj-Mohammed.[1] This sequence was to prove important. When, just before his death, Woolley came to publish the results of his deepest sounding at Ur (with the fine, clear section which we have already mentioned in another connection), it could be seen that he also had divided his Al'Ubaid pottery into three phases. The pottery of the earliest phase, fragments of which were found beneath the so-called Flood deposit, included a good deal of Qal'at-Hajj-Mohammed ware. Not much of this was illustrated in his publication; but one immediately noticed that some of it was recorded as having been *picked up on the surface at Al'Ubaid.* Surely this must mean that he had given up his excavation of the settlement at Al'Ubaid too soon. The poor wattle-and-daub shanties—"huts in the marshes", as he called them—which he

(1) Published in detail by C. Ziegler. *Die Keramik von der Qala des Haggi Mohammad.* Berlin 1953.

excavated just beneath the surface of the settlement hill, were only the decadent aftermath of a village, whose, life, if he had penetrated deeper, he could have followed back through all three phases which we found at Eridu.

Such imperfections are often found in the reasoning of one's predecessors in a particular line of research: but it is always well to remind oneself that only in the light of subsequent discoveries have they become obvious. In the present case, with so much new evidence to hand, the time seemed to have come when it should be possible to assess the combined implications of our own discoveries at the three sites: Al'Ubaid, 'Uqair and Eridu. And this I recently attempted to do in the journal *Iraq*.[1] The picture which emerges is as follows.

First to be considered are 'Ubaid and 'Uqair. Both are small village settlements, which may now be presumed to have existed all through the three sub-phases of the 'Ubaidian period, constituting the first human occupation of south Iraq. After that they were abandoned and never made use of again, except as graveyards by the Sumerians of later dynastic times. But in both cases, the religious shrine or temple attached to the settlement continued to be rebuilt and used for many centuries afterwards. In later periods in fact, its embellishment seems to have increased, perhaps in proportion to the sanctity with which its antiquity invested it. By the time of the Sumerian dynasties, as is shown by Hall and Woolley's discoveries at Al'Ubaid, it had become very considerable: and in addition, the abandoned site of the old settlement had become a burial ground, perhaps for pious individuals from the city of Ur some four miles away. In fact, in its relationship with Ur, the Al'Ubaid shrine could have become a place of pilgrimage. Its remains are still visible from the summit of the Ur *ziggurat* and it could from there easily have been visited in a day.

The situation at 'Uqair again makes a striking parallel. Here again, long after the original settlement had been abandoned the temple survived, and, as late as the Proto-literate period, was elaborately embellished with painted frescoes. In noticing furthermore that here again the site of the old settlement is covered with Early Dynastic graves, one is reminded that geographically 'Uqair stands in relation to the huge ancient city of Kutha, exactly as Al'Ubaid does to Ur. The distance between the two is an easy day's journey for a pilgrimage.

Next then, it will be interesting to consider how the ancient remains of Eridu fit into this picture. There can be little doubt that, in the pattern of

(1) "Iraq" Vol. XXII 1960 p. 23.

agricultural settlements which resulted in the 'Ubaidian occupation of southern Mesopotamia, some, like Ur, were singled out as major market-towns or administrative centres, destined on account of their size or importance to prolonged survival. After the end of what we call the Al'Ubaid period, others owed their survival solely to the sanctity of their religious shrines: and probably one of the most sacred of all was the temple of Enki at Eridu. Eridu had originated in an 'Ubaidian mound, far larger than those at 'Ubaid itself and 'Uqair: and one is at this point reminded that also at Eridu a well appointed cemetery, containing over a thousand graves, bore witness already in 'Ubaidian times to the sanctity of the temple in whose vicinity it was located. Nor, as we have seen, did this temple survive into the subsequent Uruk period as an isolated building. It was surrounded by a *temenos* at least two hundred yards square, partly filled with subsidiary buildings. In the proto-literate period these buildings, now buried up to their eaves in drifting sand, were enclosed in a retaining-wall to form the basis of a new and gigantic temple platform. The same platform, stripped of its earlier buildings, was used by the late kings of Ur as an emplacement for their *ziggurat*. Indeed, it was amusing to note that Shulgi, the Third Dynasty king who first built the *ziggurat*, was evidently aware of the traditional richness of the ancient shrine. For, before laying the foundations, he honeycombed the older buildings beneath, in search of treasure, and, to judge by the litter of gold leaf which we found in one of them, he may not have been entirely unsuccessful.

Finally one may add a word about the probable appearance of the great Proto-literate temple, the remains of which Shulgi must have removed in order to build the *ziggurat*. We were able to clear the stone sides of its emplacement, whose curious wavy facade provided a base above for the mosaic-covered half-columns of the upper storey. Only a few stones remained in place to suggest the shape of the latter: but a minute examination and recording of these enabled us to reconstruct the "portico" feature which appears in our subsequent restoration.[1] As for the temple itself by which the whole structure was crowned, no evidence of its shape remained. For the purpose of imaginary restoration we were compelled to fall back on the known appearance of the contemporary Painted Temple at 'Uqair.

Since this framework of speculation regarding the *raison d'être* of such isolated temple mounds has departed rather far from the subject of excavating technique, it may be interesting as a postscript to reflect on the frustration which Campbell-Thompson must have felt in his attempts to

(1) Seton Lloyd. *Art of the Ancient Near East*, London 1961. Fig. 218.

probe the secrets of so characteristic a Mesopotamian mound. His previous experience had been largely in Britain. His method of approach therefore, was to cover the site with small trial-holes, seldom much more than two yards square. These were indeed excavated with extreme care, as one gathers from his report, in which he meticulously records, with sketches, the depth beneath the surface and character of each sherd or small object found. But one sees now that this process could have been prolonged almost indefinitely without any prospect whatever of coming to understand the anatomy of the mound. Nor, for that matter, does it seem likely that any text-book rule of procedure which is today available would have been over-much help to a prospective excavator at a site of this sort. In our own case, flexibility of procedure and an understanding of what Campbell-Thompson called "dissolved mud brick" were perhaps the two factors which contributed most to the success of our operation.

CHAPTER IV

North Iraq: Prehistoric Mounds

During the greater part of the two previous chapters, our subject has been confined to the excavation of mounds in Mesopotamia: and this country must still occupy us a little longer. The quest for early civilizations which we have been unconsciously following, since it ran parallel to the evolution of an excavating technique, may now be pursued one stage further by turning our attention northwards towards the undulating hill-country which was once Assyria. This will still serve our primary purpose, because northern Iraq is outside the limits of the alluvial plain and excavating conditions are a little different from those in the south.

North Iraq was of course the scene of the first archaeological exploration by the pioneer excavators of the nineteenth century—Botta, Place, Layard, Rassam, Rawlinson. One could make a story, by no means irrelevant to the present subject, out of the strange, primitive methods and devices which they employed in digging mounds such as Küyünjik and Nimrud. One has seen them for instance tunnelling along the sculptured wall-faces of the Assyrian palaces, sinking occasional vertical shafts to let in the light; discarding even inscribed tablets because they did not at first know what they were. And one has seen the incredible wealth of rather damaged antiquities which they did succeed in bringing back to Europe.[1] But it is not this strange adventurous prelude to scientific archaeology which it is now our purpose to discuss. It is the sequel in the nineteen-twenties and thirties, when trained archaeologists, now supported by geologists, botanists, palaeontologists and even geochronologists, began to investigate pre-Assyrian civilizations of whose antiquity Layard and Rawlinson could not have conceived. And curiously enough, it was an excavation in the huge palace mound called Küyünjik at Nineveh which first knitted together the main threads of this research. It is also greatly to the credit of the late Dr. Campbell-Thompson, whose name appeared frequently in the previous chapter, that the historic sounding made there was undertaken on his initiative.[2]

(1) Much of this is described in S. Lloyd. *Foundation in the Dust* Penguin 1956.

(2) R. Campbell Thompson has a very detailed description of the Küyünjik mound and its history in *A Century of Excavation at Nineveh*. London 1929.

Küyünjik is a most remarkable mound. It represents the remains of a small provincial capital in Sumerian times: and before that it had probably already been a market-town at the Tigris crossing for something over two thousand years. When the Assyrians came to lay out the city of Nineveh Küyünjik offered itself as an admirable site for a group of royal palaces and temples. It was then about seventy feet high, with a flat top having a maximum diameter of about a quarter of a mile. This they packed out with mud brick to make a regular shape, revetted its sides, partly with stone, paved it on top with kiln-baked bricks and proceeded to build. Meanwhile, starting from the foot of the mound, the city itself spread northward and eastward—a vast rectangle, two and a half miles long and a mile wide, surrounded by walls seventy feet thick. Standing on the summit of Küyünjik today, one sees them as high ridges of earth disappearing into the distance, punctuated by small mounds where the fifteen gateways occurred.

Küyünjik itself was very heavily tunnelled and trenched by the many excavators who followed Layard and Rassam in the second half of the last century. But it had again been in even greater danger of another sort in the present century. From a military point of view the mound offers itself as an obvious strong-point: and during the First World War it was deeply entrenched by the Turks. There is the story regarding Campbell-Thompson, of how at the end of his first season of excavating there, he sent specimens home for analysis at the British Museum, including one which he labelled "A curious mineral". The analyst's reply read—"This is not a curious mineral: it is nitroglycerine and still explosive". Even during the last war, I myself experienced some difficulty in protecting Küyünjik when Mosul was being fortified against a German attack from the north. By the time I was allowed access to the military operations which were taking place there, I found that a Colonel of the Royal Engineers had already made a substantial excavation in the summit of the mound, in order to install and conceal a gigantic fuel tank, which would, if things had turned out less fortunately, have converted the place into a first-class military target. I found that, without ever having seen a plan of the Assyrian palaces, he had, by some miracle, chosen a depression which marked the site of one of the great central courtyards of Sennacherib's palace, and, apart from a pile of sculptured slabs, neatly stacked for removal by some illicit excavator of the last century, he had damaged nothing more than the brick pavement.

This story is pertinent to the progress of Campbell-Thompson's excavations. In choosing a site for his deep sounding into the pre-Assyrian levels

of the mound, he himself was able (by intent on this occasion), to identify and make use of the open court in front of the Ishtar temple. When the sounding came to be made in 1930, it was in charge of M.E.L. Mallowan, who had previously been Woolley's principal assistant at Ur, and the operation as recorded in his report[1] provides an interesting example of what we have called the "centrally placed vertical test." It started with a spiral staircase, up which earth was carried in baskets; but as the deeper levels were reached this became impracticable. Mallowan says—"The loose soil in the upper levels caused us to cut the sides of the pit at a sharp batter and this, together with the rapidly encroaching staircase, made the work in the last stages of the descent somewhat arduous. For the last thirty feet or so, we cut each step a yard deep and worked with a chain of not less than 50 men, who passed the baskets by hand to the top of the pit."[2] By this means the sounding was actually completed and reached the clean soil on which the settlement had been founded at a depth of ninety odd feet beneath the summit of the mound.

In order to understand more fully the significance of Mallowan's discoveries in this sounding, it may be well first to consider what he had presumably expected or hoped to find, in the light of other excavations which were then taking place in the same area. Beneath the Assyrian remains then, he would seek for some indication of an occupation contemporary with the Sumerian cities in south Iraq. Already the Germans, excavating at Ashur about thirty miles down the Tigris, had proved the extension of Sumerian culture to this province.[3] Choosing a temple dedicated to Ishtar for a vertical trial, they had penetrated into a long succession of earlier buildings, until they came to an "archaic" temple—a perfect model of a Sumerian shrine, complete with stone votary statues exactly like those found in the south. So Mallowan would hope to find and identify a Sumerian level, as a chronological point of departure for his investigation of the earlier cultures beneath. Next in importance he should find the 'Ubaidian culture, which was discussed at length in the previous chapter, or its northern equivalent. Beneath this should come the beautiful polychrome pottery of the so-called Tell Halaf culture. This also was by now well known. It owed its name to a site on the Turco-Syrian frontier, where it had been discovered fifteen years earlier by the elderly German excavator,

(1) R. C. Thompson and M. E. L. Mallowan. *Excavations at Nineveh* "Liverpool Annals of Anthropology and Archaeology" Vol. XX 1933. p. 127ff.

(2) Thompson and Mallowan *op.cit.* p. 127.

(3) Published by W. Andrae. *Die Archaischen Ischtar Tempel* in Assur. Leipsig 1922.

Baron Max von Oppenheim.[1] But it had been a chance product of von Oppenheim's dig. He had been excavating the Iron Age palace built by some obscure Aramaic-speaking people in the eighth century B.C.: and he was absorbed in recovering from its ruins the hideous portal sculptures which were to be seen in his own museum in Berlin until the bombing in 1944, when they were completely destroyed. The Tell Halaf pottery had been thrown up when the foundations of the Iron Age palace were dug, and was found by von Oppenheim in unstratified piles. It was greatly to his credit that, despite this unpromising situation, he correctly estimated the antiquity and appreciated the importance of this beautiful pottery sufficiently to publish it.

It was Mallowan himself who had recently again discovered both the North Al'Ubaid and Tell Halaf pottery, this time in their proper context. He had excavated a beautiful little prehistoric settlement at Arpachiyah, near Mosul, where he found the pottery of both periods intact among the remains of the circular —"tholos"type— buildings.[2] After this, Americans from Pennsylvania had further investigated the two periods on a much larger scale in a huge mound nearby called Tepe Gawra. And this operation deserves a further word of description, because it threw a new light on the technical possibilities of mound excavation.[3]

Owing to the generous endowment of the Pennsylvania expedition, and the long-term programme which it made possible, the two excavators, Speiser and Bache, were able to encompass the entire area of the mound's summit. Stage by stage and occupation after occupation, the whole top of the mound was removed and its height gradually reduced. I myself was at that time working intermittently at Khorsabad, about four miles away: and from the summit of our own mound I watched the changing silhouette of Tepe Gawra as the excavation continued from year to year. As its sixty-foot stature was reduced, to fifty, forty and finally thirty feet, the piles of dumped earth rose in proportion until the site of the excavation became a dark hollow, surrounded on all sides by a high barrier of earthworks. This costly excavation at Tepe Gawra was most certainly not oversubsidised. In association with the North Al'Ubaid pottery, Bache found an acropolis of beautifully designed mud brick temples, similar to those whose discovery at Eridu in the southern marshes was described in the last chapter: and beneath these he came upon an interesting circular

(1) Described in English in M. von Oppenheim's *Tell Halaf*. London 1933.
(2) Fully published by M. E. L. Mallowan in "Iraq" Vol. II 1935.
(3) C. Bache and A. J. Tobler. *Tepe Gawra*. 2 vols. Philadelphia 1950.

building of the Tell Halaf period. But at Tell Halaf his dig ended: and although he made deeper soundings, almost no clue was obtained to the nature of the preceding occupations.

So this approximately was the stage which had been reached when Mallowan came to make his sounding at Nineveh, and the auguries for its success were favourable from the start. One had almost forgotten that, before reaching the Sumerian levels, an Akkadian occupation was to be expected. The whole topography of the great Akkadian kingdom has remained to this day something of an enigma, partly on account of the fact that the capital, Agade, itself has never been located. But it has always been assumed that the Akkadians were a northern, Semitic people, whose influence spread southwards, peacefully to absorb and recondition the Sumerian city-states in the second half of the third millennium B.C. And here suddenly in Mallowan's sounding came dramatic proof that Nineveh at least had been an important political centre at the height of their power. By an extraordinary stroke of fortune, within the narrow limits of the pit, he came upon the life-size bronze head of an Akkadian king—perhaps Sargon of Akkad himself—which is now one of the great art treasures of the world.

But the Sumerian levels too were productive. Several Sumerian kings had recorded the rebuilding of temples in the city of that period and such architectural remains as were found within the sounding made this seem probable. But even more interesting was the richly painted pottery with which this occupation had commenced. In the south, Sumerian pottery is all monochrome and rather homely in appearance: but here was an exotic painted ware, often in the shape of graceful chalices, ornamented with stylised pictures of birds and animals. There were also very finely made monochrome vessels ornamented in relief. All this material was unlike anything hitherto found and for want of a better name it came to be known as "Ninevite Five", having been found in the fifth occupation-level of the sounding. It has always seemed to me that a very special interest attaches to this pottery, because it represents a whole sphere of Mesopotamian archaeology which has been considerably neglected. What was happening in this huge fertile area of north Mesopotamia while the Sumerian cities, of which we know so much were flourishing in the south? Apart from a temple which we have seen at Ashur, others hardly more than suspected at Nineveh and Sumerian cities far to the west at Mari (Tell Hariri) and Tell Brak, the upper Tigris during the third millennium is an archaeological blank. Yet many other sites producing Ninevite V pottery

have been noted. I myself found a huge triple mound north of Mosul in a survey which I made in 1938, and I short-listed it as a mound which should be excavated for this reason. There was a strange sequel. In 1956-57 a Japanese archaeological expedition (whether on the strength of my own report or otherwise), made soundings in this mound. But unhappily, ignoring my indication of where the Ninevite V remains were to be found, they dug their trenches in quite another part and were rewarded with prehistoric finds of only moderate interest.[1] The solution of the Ninevite V problem still offers a great opportunity to some future excavator.

But to return to Mallowan's sounding, which was now passing through the 'Ubaidian and Tell Halaf levels; the area was becoming increasingly restricted. By the time Mallowan had come to the crucial last stage, where he could hope to excavate the levels underlying the earliest Tell Halaf occupation, he could not hope for finds on a large scale. But, with the shaft reduced in dimensions to approximately eight feet square, his reward was by no means negligible. From pavements directly above virgin soil he recovered a basketful of potsherds, ornamented with curious incised designs. This pottery came appropriately to be known as "Ninevite One": and for the ten years which followed, these few sherds remained the only existing clue to the pre-Halaf occupation of Mesopotamia.

In 1942, an inspector of the Iraq Antiquities Department, who was assisting Land-Settlement officers in the agricultural areas southwest of Mosul, sent into headquarters some bags of surface pottery from mounds which he had found in that area. Examining one of these, my Arab colleague, Fuad Safar and I were astonished to find that it contained almost exclusively Ninevite I pottery—far more, in fact, than had been found by Mallowan at Nineveh itself. They came from a mound called Hassuna, some twenty miles from Mosul, and understandably only a few days elapsed before an opportunity was found for us to visit it.[2] The most intimidating aspect of the place proved to be its remote situation, on the extreme edge of the cultivated country west of the Tigris, with nothing beyond but the desolate Jasirah Desert, stretching away towards Hatra and the Dantesque landscape of the Wâdi Tharthar. But there was a tiny village, and nearby, at the junction of two dry river-beds, a small mound hardly more than fifteen feet high and a hundred yards in diameter. Also the surface pottery appeared to be Ninevite I, with a large number of interesting variations. So no doubt remained that the site must be excavated. By the early spring

(1) The report is published in Japanese. N. Egami. *Telul-ath-thalatha: The excavations of Tell II.* Tokyo 1959.
(2) These excavations were later published in the "Journal of Near Eastern Studies" Vol. IV 1945.

of that year we had organised an expedition and were living in a tent camp on the site.

The excavation at Hassuna is one which will always remain in my memory, as distinguished by two unusual features. One was the extreme demands which it made on one's ingenuity as an excavator; the other the disconcerting insecurity of our living conditions. The village itself seemed to be outside the jurisdiction of the nearest police-post: and beyond it was a tribal area that no one within living memory had ever attempted to administer. The Jabur tribe, who were our neighbours, had the most liberal ideas about private ownership and there was usually a good deal of shooting during the night. It was a period of war-time austerity: so the tyres of our car for instance were worth something over a hundred pounds a piece in the market, and anything else valuable had to be kept chained to our beds. Also there were curious weather conditions. One night there was a freak storm of wind, of the sort for which the district is well known; and the next morning the whole contents of our kitchen, including heavy cooking-pots, were recovered from a corn-field almost a quarter of a mile away. It can be imagined that these conditions were not ideal for patient and methodical work. But this was certainly what was required.

The conformation of the Hassuna mound was an extraordinarily interesting one, since it perfectly illustrated the phenomenon which we have previously described as a "shift in the focus of occupation." It was possible to understand this before starting to excavate. Judging from the preponderance of "Ninevite I" pottery on the surface, the earliest occupation of the site was on the east side at the apex of the triangle formed by the convergence of the two little river-beds, and it extended from there up to the summit of the mound. But the lower slopes of the mound on the west side were covered with polychrome sherds of Tell Halaf ware, which one assumed to be considerably later in date. One could see therefore that, restricted by the enclosing banks of the two streams, the village had in later times expanded westwards and that traces of its latest inhabitants would be found low down on the western flank. And this was in fact exactly how it proved when we came to excavate. The "Ninevite I" occupation had first created a tiny mound at the apex of the triangle: but the Tell Halaf village, which was bigger, had spread westwards down onto the level ground behind. On the west side therefore, the earliest remains were deeply buried: but to the east they lay directly beneath the surface.

So it was to the east that we began excavating: and here we ran straight

into some of the most difficult wall-tracing that we had ever experienced. There was a cluster of small primitive houses, but they were built of *pisé* clay, without plastering, and the material of which the walls were made proved almost indistinguishable from the fallen debris which filled the rooms. It needed all the ingenuity of our best wall-tracers to recover the plan. But when this had finally been done, we were rewarded by the recovery of a neat village lay-out, including the two tiny farmhouses, a reconstruction of which has since frequently appeared in archaeological and other publications. Their floors were covered with broken pottery of the "Ninevite I" type and we accumulated an impressive repertoire of attractive vessels, together with all the paraphernalia of primitive agriculture. Furthermore, the levels beneath seemed to illustrate most vividly progressive stages in the establishment of a settled community.

After a short occupation which one could describe as Neolithic, since the decorated pottery had disappeared and only plain burnished vessels seemed to be in use, we came to a level clearly representing the first settlement of all. One received the impression that these first inhabitants were still a semi-nomadic people: for they had apparently not even built themselves houses. All we found on the original clean soil was a pattern of domestic hearths, over which some rather rudimentary form of shelter must have been built, perhaps of branches or woven screens. Around each hearth was a standard complement of domestic equipment; large coarseware containers for milk, storage-jars sunk in the ground, chipped flint hoes, polished stone adzes and bone implements. But there were also sickles with flint teeth set in bitumen, to suggest that they had already begun to reap the wild barley which still grows in the neighbourhood. When this camp-site was temporarily abandoned, one individual, perhaps a sick man, had remained behind, and we found his skeleton crouched besides the ashes of a fire. It is now in the Iraq Museum.

During the Hassuna excavations, in spite of the rather primitive conditions under which we were living, it was necessary to devise some effective routine for dealing with the large volume of pottery which was continually emerging. After being carried to the camp each evening and carefully washed, it was laid out in what American excavators call a "sherd-yard", divided by string into sections according to its level and provenance. In this way, not only could complete vessels be reconstructed from the marked fragments, but it proved possible to make a "sherd-count", registering the actual number of fragments of each different ware. When afterwards we faced the task of deducing historical inferences from the evidence

provided by changes in pottery technique, we even found that it could be formulated in the shape of a mathematical diagram. This was an archaeological innovation which proved extremely effective, and it has since been very generally adopted by other excavators. In such a diagram the changes in ceramic fashion can be followed from level to level and the actual quantity of sherds can be taken to indicate a period of maximum popularity for each ware. It is the clearest form in which the outcome of such an analysis can be summarised.

So the pottery of the earliest level at Hassuna replaced the "Ninevite I" ware as a symbol of the earliest inhabitants of Mesopotamia. And now considerable interest was aroused in the possibility of attributing an actual date to the period when these nomads transformed themselves into farmers. The only method by which, in those days, a calculation of this sort could be made was by correlating the passage of time with the accumulation of debris on the inhabited site and the consequent rise in occupation level. And this was generally considered to be of doubtful reliability. It was therefore with some sense of temerity that we ourselves did on this occasion actually make the attempt and publish the result. First, then, a *terminus ante quem* was necessary for the calculation, and for this we took the fairly well-dated beginning of the Sumerian dynasties. Next, one had to consider the actual depth of occupational debris by which each of the later Chalcolithic phases was represented at other sites, such as for instance the Tell Halaf culture at Tepe Gawra, the 'Ubaidian at Eridu and the Uruk period at Warka. If one added together all these deposits and superimposed them on the seven metres of the Hassuna mound, it would presumably give one an approximate total for the depth of deposit created by an imaginary occupation lasting from "Hassuna Ia" till the beginning of the First Dynasty of Ur. In this case, the result of the sum was an aggregate of sixty-two metres: so, taking the average deposit which we thought could be created by a single generation at fifty centimetres, we reached a result which suggested that the first occupation of Hassuna must fall somewhere in the fifty-first or fifty-second centuries B.C. At the time, we ourselves had no great confidence in the accuracy of this calculation and proposed it merely as a basis for further argument. It has therefore more recently been both gratifying and a little surprising to hear of a substantial confirmation from another source. In 1957 an expedition from the Chicago Oriental Institute returned to Hassuna to extract from our old trenches material for a Carbon/14 test of the sort which had of course not yet been invented when we excavated there. The date arrived at for the fifth occupation level,

when the settlement was well established, read -5080 B.C., (plus or minus two hundred years).[1]

These remarkable finds at Hassuna were made, as we have said, under war conditions, and some difficulty was experienced in arranging for their adequate publication. Much invaluable assistance in editing our report for an American journal,[2] was eventually given by our colleague Professor R. J. Braidwood of the Oriental Institute, and when the war was over, he was one of the first foreign archaeologists to resume the same line of research in North Iraq.

In 1945 Braidwood had started to plan a long-term and painstaking investigation of the phase in man's history which the late Gordon Childe had called the "Neolithic Revolution". By this he meant the transition from the cave-dwelling and food-gathering economy of the Palaeolithic period to the cultural level of village farming communities. Much reflection on this subject had created a new focus of interest in what Braidwood described as the "hilly flanks of the Fertile Crescent"; that is to say the piedmont country which swings eastward from the Lebanon through the northern fringes of Syria into Iraqi Kurdistan. His reason for this choice is perhaps best explained in his own words. In a subsequent report on his operations he says:[3] "Within this hilly-flank zone, there occur in nature a remarkable constellation of the very plants and animals which became the basis for the food-producing pattern of western cultural tradition. Nowhere else in the world were the wild wheats and barley, the wild pigs, cattle and horses to be found together in a single natural environment. Such is still the case (save for the extinct wild-cattle and horses), in the range of elevation and rainfall concentrations which the hilly-flanks zone represents. The coincidence of the distribution of such early village sites as were known, within this same zone itself was also very striking when mapped."

Braidwood's proposal then, was first, to search for excavatable sites in that area, which might throw further light on the earliest settlers at Hassuna and their antecedents; sites, that is, at which he could work downward from the Neolithic. But he was also encouraged by a further circumstance; namely that this piedmont area was known from investigations made before the war to contain a promising concentration of caves and open shelters occupied in Palaeolithic times: and in the upper levels of

(1) R. J. Braidwood and B. Howe. *Prehistoric investigations in Iraqi Kurdistan.* Chicago 1960. p. 160.
(2) S. Lloyd and F. Safar *op.cit.*
(3) Braidwood and Howe *op.cit.* p. 3.

these sites, he would also be able to work in the opposite direction; upwards from the end of the Old Stone Age. It is also much to his credit that his plan of campaign extended beyond the realm of mere digging. It will already have been gathered from the wording of his report that his interest was also in the domestication of plants and animals and in all the other environmental influences which had helped to bring about the Neolithic Revolution. His expedition then, in addition to mere archaeologists and specialists in the lithic industries of early times, was to include both botanists and zoologists. What he envisaged, again in his own words, was "...not the familiar, old-fashioned archaeology of digging royal tombs for fine-art museums, but an 'idea-archaeology', aimed at broad culture-historical problems, in which antiquities as such are meaningless save as tools for understanding the ways of mankind."

All this led in due course to the field-activities of what came to be called the Iraq-Jarmo project of the Oriental Institute. It would be out of place here to dwell at any length on the expedition's multiple activities among the Palaeolithic caves of Iraqi Kurdistan, because cave excavation lies outside the sphere of our present subject. Braidwood's own excavations on the other hand, at the site which gave the project its name, are of great interest, especially in regard to the unusual archaeological problems which they presented. First then, let us consider how the site came to be chosen.

Of the two sites which we recommended to Braidwood, Matarra, a small mound south of Kirkuk at first proved a disappointment. After a season of excavating, no traces had been found of a pre-Hassuna occupation and the settlement seemed to differ only in minor respects from that at Hassuna itself. Jarmo however seemed likely to be very different in character. In 1945 Fuad Safar and I had paid a memorable visit to it on a hot May afternoon—three hours on horseback from the nearest motor road—a tiny Kurdish village far up in the foothills above the town of Chemchemal. Near the village, at a spot pointed out by the inhabitants to our inspectors, there was a curious natural formation. (PL. 26) A shoulder of hillside, composed of silt and conglomerate had been cut into and eroded by the flood-water of a deep wadi, leaving a cliff about a hundred feet high. And in the brow of this cliff one could see, perfectly delineated, the stratification of a human settlement which must have stood on its summit, now covered by a sparse crop of wheat. Among the wheat we were able to collect a handful of very weathered and nondescript potsherds. But what interested us more was that, by hanging over the actual cliff-edge, one could see that the pavement lines marking successive occupations seemed

to be covered, not with pottery but by a deposit of edible snail-shells, among which was a good sprinkling of microlithic flints. A combination of these two features suggested to us the still rather nebulous and ill-defined label "Mesolithic", and it was this which we tentatively applied to the site in our subsequent report.

Late in 1948 Braidwood made a jeep-road to Jarmo and set up camp there. The sounding which he made led to a full-scale excavation in 1950 and another in 1954. The overall results of the expedition have now been very thoroughly published, and it should be possible fairly to assess the contribution which they made to solving his major problems. In doing so, it is impossible to avoid the conclusion that he himself must have found them disconcertingly equivocal. He did indeed uncover what has since been described in his own words as—"a very early but fully fledged village farming community." The site contained about a dozen architectural levels, creating a deposit about twenty feet deep on top of the hill. Pottery having something in common with the deepest levels at Hassuna but less indication of technical competence, was fairly plentiful in the upper third of this deposit, but afterwards ceased to be found. He estimated that the total number of houses in the village at any one time did not exceed twenty-five and that the population had amounted to no more than 150 people. He thinks that the lifetime of the village may have been 250 years: and the most reliable Carbon/14 date which he afterwards obtained gave a reading of approximately 6750 B.C., which suggests a far greater antiquity than Hassuna. It is however in the realm of archaeological technique that one finds the main interest of the excavation and there is much to be learnt from Braidwood's own account of his procedure.

The sounding which he started in 1948 was to be regarded as a "test in depth" and it presented no particular practical difficulty. Two areas about fifty feet square were chosen, so that the earth could simply be dumped over the side of the Wâdi: and during the first and second seasons they were both carried down stage by stage to the clean soil beneath. The moment next came when it would be desirable to extend the excavation horizontally, and it was then that a variety of complications were encountered. "As the third season began", Braidwood says, "we rashly assumed that we had sampled Jarmo in depth. Hence we determined to attempt an exposure of the village plan in breadth in the uppermost levels. Our experience indicated that it would take our normal staff and a crew of fifty diggers approximately ten successive digging seasons to strip the whole surface of Jarmo at a safe pace, and then only to a depth of about two metres at the

most. To exhaust the mound completely would certainly have taken over twenty-five digging seasons. Obviously a short cut was necessary."

The short cut which Braidwood adopted can be seen in his plan and equally clearly in photographs of the site after it was done. (PL. 27) It consisted in covering the whole area with a five-metre grid and then sinking a shaft two metres wide in the centre of each square. In terms of the earlier excavations which we have described in previous chapters, this would seem to be a compromise between the uniform *'suchgrabung'* of the German school the more haphazard test-pits of Campbell-Thompson: and it seems to have fallen about halfway between the two in its degree of effectiveness. Braidwood says—"Unfortunately, what this test system showed us most clearly is that there are no short cuts for examining what goes on underground. The intermittent sections have still to be studied in detail, but it appears that the extrapolation of completely meaningful sections which we had hoped for will be impossible. The underlying strata of archaeological sites may pitch and toss in ways which their present surface contours seldom suggest; the conventional lecture-hall analogy of archaeologists that the layers in a mound are like the layers of a cake is a vast over-simplification. In Jarmo the pitching and tossing seemed to be excessive in some portions of the mound." In a word, Braidwood had added to the total of archaeological experiments which have been made in our time, one which will perhaps not be repeated. But it is a sobering reflection that if one were asked to suggest an alternative procedure at a site of this sort, one could only feel constrained to protest that the strategy of excavating depends on intimacy with the special circumstances. And perhaps the latter would have suggested that the whole costly operation was unlikely to be justified by the results.

The Jarmo excavation was no more than a constituent part of the complex enterprise which Braidwood had embarked upon. In the light of his undoubted successes elsewhere, the absence of spectacular results from that particular site must have seemed a very minor disappointment. In the published report of the expedition's work, other remarkable accomplishments are recorded, particularly in the realm of natural science. The zoologists for instance have obtained new information about the domestication of animals which makes most stimulating reading. It is discovered that the wild sheep was not at first domesticated for the sake of its wool, but for its meat and hide. Interest in its wool came as a bi-product. Then there are discoveries in connection with the domestication of grains and other food plants, summarised in what is called a Palaeoethnobotanical

Report. Jarmo is proved to be, as Braidwood suspected, within the area where the earliest experiments in cereal domestication were made. Wheat comes first, some of it still bearing a resemblance to the wild wheat of the present day, and other forms showing signs of domestication under artificial conditions. Barley comes into cultivation as a result of its presence as a weed in the wheat-fields. Then there are non-cereal food plants, such as peas, lentils and vetches, apparently domesticated, and others such as pistachios and acorns which seem still to be evidence of food-gathering habits. All this information was obtained from small finds incidental to any Near Eastern excavation, of a sort which often receive the most inadequate study or are even carelessly discarded. Now for the first time they could be examined in their original setting by properly qualified specialists.

Generally, there is no doubt that Braidwood's enterprise made a firm beginning in the newly widened field of research which he himself had conceived. Meanwhile his archaeological discoveries were being supplemented by excavators of other nationalities in neighbouring countries, particularly at Jericho in Jordan, where Dr. K. Kenyon had penetrated the remains of a "pre-pottery" Neolithic town perhaps corresponding in time with the beginnings of the village at Jarmo.[1] But almost a decade was to elapse before a setting was finally found in which the real prodigies of Neolithic accomplishment could be examined on a broader scale and properly correlated with their subsequent Chalcolithic development. And when this happened, it was not in Jordan nor in Mesopotamia, nor even in Braidwood's hilly-flanks zone, but in the western uplands of Anatolia; a country which until recently had hardly been credited with a formative civilization earlier than the third millennium B.C. It is to the mounds of Anatolia that we must now turn our attention.

(1) Reported in the "Palestine Exploration Fund Quarterly" 1954-'57.

CHAPTER V

Excavations in Anatolia

The name Anatolia is today generally applied by its inhabitants to the whole of Turkey-in-Asia from the Sea of Marmara to Lake Van. Throughout most of this broad peninsula, and especially around the fringes of its high central plateau, ancient mounds are almost as plentiful as in Syria or Iraq; and if they are less conspicuous it is perhaps the character of the landscape with its mountains and forests which makes them so. One area is exceptional in this respect. The provinces bordering the Aegean Sea are indeed studded with the remains of ancient Greek cities: but those which are not buried beneath the alluvium of the river valleys have survived rather in the form of ruinfields, scattered with the remnants of stone buildings. Throughout the length of the Aegean coast, from Lycia to the Troad, actual mound formations are extremely rare. And this confers a certain distinction on the otherwise unpretentious hill called Hissarlik, which today is generally considered to be the site of the city of Troy.

If, in the context of our present subject, it is occasionally necessary to refer to the ill-directed beginnings of archaeology in the Near East where Anatolia is concerned, the natural point of departure is the site of Troy and the excavations begun there by the great amateur archaeologist, Heinrich Schliemann. But of Schliemann it must be said at once that one's admiration for some aspects of his character inhibits the most obvious criticisms of his ineptitude as an excavator. As I have written elsewhere—"It is not by his shortcomings that he is to be judged. The personal quality which immortalised his name was the devout and childlike faith with which, in spite of all ridicule, he clung to an actual historic foundation for the Homeric poems and the Trojan war. And this was in the end victorious over the accumulated scepticism of those more erudite than himself." It is not then our purpose here to ridicule Schliemann's methods nor to lament the disaster which entrusted the first stratified excavation in archaeological history to the hands of an "absolute beginner". Our interest will be focussed on the actual mound called Hissarlik and the deductions which either have or have not been made from its excavation.

But before discussing these, it may be well to summarise very briefly the

actual sequence of archaeological operations which have taken place at Troy during the past ninety years or so. Starting in 1870, Schliemann worked there by himself intermittently until 1878. In 1873, when the isolated pits he had been making began to create a situation of impossible confusion, he set about digging his famous "Great Northern Trench", which cut clean across the summit of the mound. Stratum after stratum of building remains appeared, some of them bearing obvious indications of destruction by fire, but there seemed to be nothing which could be satisfactorily isolated and identified as Homeric Troy. In search of some more tangible result, he carried the trench down through the full fifty feet of occupational deposit until he reached the bare rock underneath. And here, because he could not go any further, he recorded the rather poorly built walls of the earliest settlement, now known as Troy Ia. From an architectural point of view, that was about all that he did record. It was not until he extended the trench laterally to the west that the famous gold treasures came to light, which led him to identify the remains of an Early Bronze Age settlement as "Priam's Ilion". And needless to say, this chronological error of about one thousand years was a good deal less ridiculous in the eighteen-seventies than it would be today.

In Europe meanwhile, criticisms of his methods had become rather widespread, and where the provenance of his finds was concerned there began to be some reluctance to accept his own unsupported testimony. From 1878 onwards therefore, he willingly accepted the collaboration, first of the two German scholars, Virchow and Burnouf, and later of Dorpfeld, who himself continued the excavations for four years after Schliemann's death in 1890. By that time Schliemann had recognised seven successive settlements, which he described as "cities", and Dorpfeld increased this number to nine. It would have been surprising and a little disappointing for Schliemann to have learnt that six at least of his "cities" pre-dated the "Homeric" remains and that little more than the outer walls of Homeric Troy any longer existed. The fact, as we now know, was that Alexander the Great, in his pious anxiety to have the place suitably rebuilt, had first of all levelled off the top of the mound, thereby removing all trace of what the walls had contained.[1]

Nothing further was done at Troy until 1932, when the University of Cincinnati decided to reopen the excavations. And now indeed one saw the changes which had taken place in the basic approach to field archae-

(1) Schliemann's own account of the excavations is given in *Troja*. London 1884. C. Schuchardt has a more comprehensive account in *Schliemann's Excavations* London 1891.

ology during the years which had intervened.[1] C. W. Blegen, who was in charge of the new expedition, planned to undertake what he himself described as "an exhausting and painstaking re-examination of the whole site"; and, as though determined in advance that the object of his excavation should be patently contrasted to that of his earliest predecessor at the site, he insisted that he should be "under no compulsion to recover objects of startling or sensational character with high publicity value." His policy was to re-excavate with proper stratigraphical care the *insulae* of untouched mound which still survived within the pattern of the earlier excavators' trenches: and in this way he hoped to substantiate the chronological framework of Dorpfeld's nine principal building-levels. The whole operation itself and the six heavy volumes in which its results were published are a monument of industry and painstaking accuracy. The stratigraphic sequence which they provided, remained until recently the only archaeological criterion by which the prehistory of western Anatolia could be judged.

But now let us look for a moment at the combined outcome of all these investigations at Hissarlik. What sort of a city do they show Troy to have been? And what, broadly speaking, had been its history previous to its final destruction at the end of the Bronze Age? During the period of Blegen's excavations in the nineteen-thirties, so great was the reaction against Schliemann's misguided romanticism, that it became almost improper to mention the Homeric associations of the site: and throughout the many hundreds of pages of the Cincinnati publication, almost the only reference to the subject is for the purpose of establishing that the first sub-phase of the seventh settlement should correspond chronologically with the traditional date of the Trojan War. But even this chilling blast of academic realism could not quite dispel the glamour which surrounds the ruins at Hissarlik. And for visitors to the site with less rigorously disciplined minds than Blegen and his excavating party, it is difficult to avoid the impression that still, in Shelley's words—"the ghost of Homer clings around Scamander's wasting springs." So let us now consider the mound itself—or rather, the remains of it which have survived after patient dissection by five famous excavators. Let us start with its situation.[2]

Approaching from Çanakkale, the road swings inland among the foot-

(1) C. Blegen's report on the Troy excavations was published in four volumes, each of two parts, (Princeton 1950, '51, '53 and '58).

(2) Everything connected with the situation of Troy is discussed in great detail in Walter Leaf's *Troy: A Study in Homeric Geography*. London 1912.

hills, and then brings one out onto a shoulder of rock, from the edge of which Hissarlik looks across the Scamander Valley to the Aegean Sea. There, before one, is the Plain of Troy and beside the mouth of the river the traditional site of the Greek camp. The rock itself rises hardly more than fifty feet above the plain and the mound perhaps adds another forty or fifty feet above that. One cannot however fail to notice the commanding position which it occupies, with its steep escarpment on three sides. And indeed, from the summit one is faced with the most remarkable panorama, not only of the maritime plain where the Homeric battles were perhaps fought, but of the Aegean and the Dardanelles beyond. Here, with great clarity one sees much of the setting of the Gallipoli campaign in the First World War. Out beyond Çanakkale to the right, cliffs draw in towards the famous "Narrows", where the British Navy so disastrously failed to break through into the Marmara. Directly before one is the Tekke Burnu or tip of the Peninsula, and east of it the sites of the landings, where the Anzac troops and their Turkish opponents fought so bravely. Away in the mists to the left lies the island of Imbros, which was used as a base for the operations.

In an archaeological context, all this does give one the impression that an ancient fortress on the spot where one is standing could have had an important function in controlling the trade route through the Marmara: and it brings one back again to considering the character of this city at Hissarlik. And here immediately one is faced by the strange paradox—that, in the proper sense of the word, Troy was not a city at all. The main subject of contention in this respect is its size. The first settlement on the site was no more than a hundred yards in diameter. By the time that Troy VIIa, the "Homeric" city was built, the area enclosed by the walls had expanded to some extent, but its diameter had no more than doubled. If we are to understand the full implications of these dimensions, it may be useful to recollect the actual acreage of certain other ancient cities. Selected more or less at random they are as follows:

The city of Nineveh (within its walls)	1,600 acres
The city of Khorsabad	640
Hattusas (the Hittite capital at Boghazköy)	415
The citadel at Hattusas (Büyükkale)	8
Homeric Troy (Troy VIIa)	5
The second settlement at Troy	2¼

So the fortified enclosure which is thought to have been the objective of the Trojan expedition was in fact hardly more than the size of Kenilworth Castle. As one supposes it to have contained the palace of a ruling prince

and quarters for his retainers, it would at no time have been possible for it, in addition, to accommodate, say two thousand armed men. On the other hand, it is clear from the Homeric description that the Trojan army and its allies must have comprised some tens of thousands; far more in fact than could actually have been contained within the walls. It has sometimes been suggested that Hissarlik was merely the inner acropolis of an outer city extending over the high ground to the east. But if so, it is strange that trenches made by the various excavators have failed to reveal traces of it. Also, the outer city could certainly not have been walled; whereas the Iliad depicts the whole Trojan force confined within the fortifications and the Achaeans circulating freely outside. The earliest historical picture we have of Troy is that of Demetrius of Scepsis, who described it in the second century B.C. by the Greek word *komopolis*—that is, too big for a village and too small for a town.[1] But probably the wisest comment on the subject was made by Thucydides, who said—[2] "If we are to judge by Homer, who as a poet might be expected to exaggerate, the Trojan expedition, though small by modern standards, was at least greater than any that preceded it." The most reasonable conclusion would in fact probably be that Troy was neither a city nor a town, but something more nearly resembling a castle or fortress—a very strong fortress indeed by Homeric standards, or at least strong enough to justify some of the epithets applied to it in the Iliad, such as "great citadel", "well-walled", "high-gated" and so forth. This is borne out by its surviving remains; heavy walls of dressed stone, as finely built as any dating from later periods, and two or more massive gateways.

When I visited Hissarlik for the first time in 1955, I went there with my colleague Mr. James Mellaart, who was also unfamiliar with the actual ruins. We had taken with us Dorpfeld's plan of the excavations and part of the American publication, so that we could study in detail the architectural remains in the numerous trenches. But we still found it extremely difficult in this way to piece together any intelligible picture of the fortress at any one period. The reason for this was not far to seek. Among Professor Blegen's drawings there is no plan in which he has attempted to combine the walls which he himself discovered with those in the scrappy, but fairly accurate plans of the earlier excavators. Far less had he attempted to amalgamate the total of walls now recorded in any one level, into some sort of reconstructable plan, covering the whole fortress area. This seemed to us

(1) Quoted by Leaf in *op.cit.* p. 57.
(2) Thucydides. i. 10.

an omission which might usefully be repaired, if only for the benefit of other visitors to the actual site: and on returning to Ankara, Mr. Mellaart determined to make the attempt himself. He did this in an article in the journal *Anatolian Studies*[1], sorting out the total recovered plan of each settlement, and clarifying it where he could, by suggesting hypothetical reconstructions for the blank spaces in between. This article and the drawings which went with it seemed genuinely to throw new light on the character of the fortress: and one of the latter—a plan of the settlement known as Troy IIg, in which Schliemann found his famous "treasures" and which actually dates from the middle of the third millennium B.C., proved to be of rather special interest.

Taking its place as the central feature of the whole plan at this period, is a great barn-like structure, twenty-five feet wide and almost sixty long, which greatly excited the classical world when Schliemann found it, because it seemed to be a very early example of the so-called Greek *megaron* (a rectangular hall with an open porch, from which in much later days the Greek temple developed). What we ourselves found difficult was to agree with Professor Blegen in thinking of this building as a "royal palace" and of all the other buildings round it merely as the dwelling-houses of his subjects. For one thing, there were found in it no traces whatever of any domestic appointments or evidence of normal family life. Also there was the question of the actual scale. When one remembers once more that the roof of the *megaron* actually has a span from wall to wall of almost twenty-five feet, it is easier to gauge by comparison the size, for instance of adjoining buildings such as the unit marked "C" on the plan, to the southwest of the "palace", and to realise that this in itself is large enough to make a comfortable residence for a ruling prince. One is then reminded that in fact, of the groups of precious objects justifiably described by Schliemann as "royal treasures", all but one were found buried actually within the area of this particular building. It then becomes easier mentally to transfer the title of "royal palace" to this building "C" and to think of the great *megaron* as a public assembly-hall or court of justice; an identification which had indeed occurred to Schliemann himself.

Mellaart also found it possible to divide up the tangle of walls in the main part of the town into "insulae" representing individual private houses. And while doing this he noticed that, in many cases, the central element of the house-plan was a unit consisting primarily of hall and porch, like an elementary *megaron*, onto which subsidiary rooms had been

(1) Vol. IX 1959 p. 133.

added at the side. He found this to be even more conspicuously the case in Professor Brea's contemporary town-plan of Poliochni in the island of Lemnos.[1] And he concluded that, in these parts, most of the domestic plans of later times were ultimately derived from an original hall-and-porch form with certain additions. And here again was a curious situation. For classical archaeologists, though they had always till then insisted on the Greek origin of the *megaron*, never seemed to have found it incongruous that far the earliest example of this architectural form (which occurs in Troy I) should have been discovered on the mainland of Anatolia. This one must attribute to a widespread and perhaps understandable tendency among them to think of "Asia Minor" as a geographical annex to Greece. The Anatolian origin of the *megaron* (and so more remotely of the Greek temple plan), has in fact now been finally established, first by a Turkish archaeologist's[2] discovery of a perfect *megaron*, dating from the Early Bronze Age, as far west as Kültepe in Cappadocia and by finds which we shall presently mention at Beycesultan in the Meander valley.

But these matters were incidental to the discoveries at Hissarlik. There remains little doubt that we should think of it as an important fortress, commanding the entrance to the Dardanelles as early as the third millennium B.C. Speaking of the second settlement, in the article to which we have referred, Mellaart said—"The possibility that the destruction of Troy II marked the end of a powerful kingdom, which had controlled the land-road into Anatolia (via the Thracian Chersonese), and the seaways into the north Aegean, should be seriously considered."[3] Later this proposal was most strikingly justified by his own publication of the so-called "Dorak Treasure", which confirmed the existence of just such a kingdom in the Troad at the time in question.[4] There now seems every reason to believe this: nor is it less probable that such a kingdom survived into Homeric times. As for the enigma of Hissarlik's modest acreage, one is tempted to suggest that this kingdom had a capital city elsewhere in the Troad, and that Troy was merely its most important outlying fortress. In that case it would have been natural that the royal family should hasten to its defence when news came of the Achaean attack.

In the catalogue of very ancient Anatolian cities, Troy was unusual in several respects. In the first place, it was as far as one knows the only city

(1) A. Brea's reports on excavations at Poliochni were published in "Bolletino d'Arte" from 1952 to 1957.

(2) T. and N. Özguç for the Turkish Historical Society.

(3) Mellaart *op.cit.* p. 162.

(4) Published in the "Illustrated London News" on 28/11/59.

which, from Early Bronze Age times onwards, had walls built of dressed stone. One thinks immediately of the magnificent enceinte of fortification and stone-built temples at Hattusas, the Hittite capital in eastern Anatolia. But these were of course built in the Late Bronze Age, when imperial expansion had familiarised the Hittites with the stone architecture of less backward peoples in Egypt and elsewhere. And, for that matter, the great four-mile circuit of city-walls with which Supiluliumas surrounded his capital at Boghazköy, had no more than a sub-structure of stonework ten or twelve feet high. Above this they consisted of mud brick reinforced with timber, as indeed was the case with the earliest Trojan walls. And thereby we are introduced for the first time to a very distinctive peculiarity of Anatolian architecture, by which the task of an excavator in that country is often greatly complicated.

Today in Anatolia there is still a plentiful supply of timber; and there is reason to think that in ancient times its central plateau and mountains were even more heavily forested. But the country has also been subject at all periods to frequent and devastating earthquakes. It is perhaps a combination of these two circumstances which has determined the form of building construction most generally favoured in what is now Turkey from the earliest times to the present day. This was a timber frame-construction, very similar to that which in England today is known as "half-timber". In ancient Anatolia, the panels between the structural timbers were filled with sun-dried brick. Today the system is practically unchanged except that kiln-baked bricks are often used. Actually it was perhaps not in Turkey but at Knossos in Crete that this form of construction was recognised for the first time, by Sir Arthur Evans. In the Middle Bronze Age "Palace of Minos", the system seems to have been so generally accepted as a sound building principle that the Cretans made no attempt to conceal it. Where the faces of brick-and-timber walls were afterwards plastered over, they would outline the position of the structural timbers in paint, as a part of the formal system of decoration. But the Hittites of Boghazköy also used an identical system. And when the German excavators at the beginning of the present century were recovering tens of thousands of clay tablets from the archive rooms of the palace on the citadel, they were at first greatly puzzled to find that the walls of these chambers seemed to consist of free-standing panels of brickwork with empty slots in between. The latter of course were gaps where the upright posts of the timber framework had stood, as they afterwards realised, when charred fragments of them were found still intact. Later, American archaeologists excavating

a Syro-Hittite palace at Taynat on the Orontes, found a building where the whole complicated framework of the walls had survived in the form of well-preserved charcoal; and they were able to reconstruct it in detail.

This mention of a site on the Orontes in Syria may serve to remind us of an interesting geographical circumstance. The outline and extent of the countries in which this "half-timber" construction is practised, will be found on the map to correspond most strikingly with the limits of the geographical area most affected by earthquakes. A hundred miles east of Taynat, at Carchemish on the Euphrates for instance, Woolley and Lawrence found no traces of timber in the walls, nor was it ever used in Assyrian or other buildings in Mesopotamia, where the recorded incidence of earthquakes is negligible. The explanation need not be inferred from equivocal archaeological arguments. It is a matter of common knowledge to modern architects in that part of the Near East, that the traditional frame buildings of Anatolia have an impressive elasticity, which often protects them from total collapse in the severest earth-tremors. There is little reason to doubt that the system was originally adopted and has persisted from the earliest times for this reason.

But to turn once more to the actual practice of excavating. It can be well imagined how greatly this predilection for timber construction complicates the normal procedure of wall-tracing, and produces phenomena which are at first difficult to understand. We have already seen the German excavators puzzling over the walls of the archive chambers at Boghazköy. Here of course the buildings had been destroyed by fire and the timber of the framing reduced to powder; so that only the foundations and the brick filling remained. And this in itself is by no means an unusual situation. For an outstanding fact about half-timber buildings (as Lords-of-the-Manor in Tudor times must have known to their cost), is that they are disastrously inflammable. Once they catch fire, the walls, as well as the roof, make fuel for the flames.

A powerfully impressive instance of this sort was to be seen in the Middle Bronze Age palace which I myself excavated at Beycesultan in the Meander valley.[1] This building in some ways resembled the Minoan palaces of Crete; a vast complex of chambers rather more than two hundred feet square, grouped around a central court and covering the whole eastern summit of the Beycesultan mound. Such evidence as we have suggests

(1) Preliminary reports on these excavations were published in "Anatolian Studies" from Vol. V 1955 onwards. A first volume of the final publication, S. Lloyd and J. Mellaart's *Beycesultan I* appeared in 1962.

that it was built in the XIXth century B.C., during the short time that this city was capital of the state called Arzawa. The form of construction was very much on the lines we have been describing. The walls were built up to about three feet high in stone and above this came timber framing, filled with mud brick. There was also an upper storey which may have consisted almost entirely of wood. But in this case the stone sub-structure itself was also reinforced with wooden beams; and even the foundations, as we discovered at a late stage in our excavations, consisted of huge logs, laid transversely to the line of the wall itself about three feet beneath the ground. Inside the building there was a forest of columns, as well as door-linings, panelling and other fittings, all of wood. We calculated that more than four thousand tree-trunks must have been used in the part of the building which we excavated alone. Whole mountain-sides must have been deforested to provide the timber.

To say that this building was "burnt down" seems almost an under-statement. We think that in approximately 1750 B.C., the Arzawan people had news of an approaching Hittite army, long enough in advance for them to be able to evacuate the palace: for not a trace of furniture, pottery or any other human possessions remained in the chambers which survived. Whether they set the palace on fire themselves or left it to be destroyed by the Hittites we shall never know. But we do know that there was a strong west wind blowing across the summit of the mound at the time, and there is no doubt about the thoroughness with which it was destroyed. One of our first search-trenches in 1954 brought us up against its ruins in the south-east corner, and for some days Mellaart and I thought we must be dealing with some tremendous brick-kiln or lime-burning installation. The heat had been so great that in many cases the brickwork had actually melted and survived in the form of huge vitrified lumps. This, in fact, is a phenomenon which one has seen before, in Iraq, on the summit of the *ziggurat* at Birs Nimrod (Borsippa) which is traditionally considered to be the ruins of the Biblical Tower of Babel. But there one is compelled to assume that the "tower" must have been repeatedly struck by lightning in some tremendous electric storm. For the solid brick work has vitrified like glass, and great masses as big as ice-bergs are split off and tumbled at all angles. But the phenomenon one had not seen previously to the Beyce-sultan excavations, was the effects of fire on a stone structure reinforced with wood. In many places the stone had simply calcined into lime, so that piles of white powder were all that remained. (PL. 29)

Under these circumstances, the difficulty which we found in tracing and

reconstructing the walls can well be imagined. This was once more a case in which one can see that no system of procedure devised by the writers of text-books on the subject would have been in the least practicable. One had simply to exercise great patience in the gradual removal of the fallen and decayed material: then to disentangle and study what remained, in order to understand the principle on which the building was constructed. There were at least two features which made this unusually frustrating. One was the poor quality of the actual pavements in the rooms, which simply consisted of beaten earth. In some of the residential parts they were covered with a fairly thick layer of carbonised rushes; but in the main reception-rooms there was no covering at all: and one was compelled to assume from various indications, that some sort of carpet or woollen fabric had been laid over it (and of course removed before the fire). Even a regular pavement of beaten earth would, in these cases, have made matters more simple, since, by outlining its edge, one could have fixed the position of the walls, which actually no longer existed. But here one was up against the extraordinary method of constructing the foundations, to which we have already briefly referred, and which was really only partially explained after our sixth season of excavating.

The following, then, seems to have been the method of procedure adopted in starting the main walls of a public building. First a trench was dug, about three times the width of the intended wall and two or three feet deep. Across the bottom of the trench, at right angles to it, heavy logs of wood were placed side-by-side, and on top of this the stone foundations of the wall itself were laid, occupying the central third of the trench's width. One would then have expected that the parts which remained open on each side would have been filled up to pavement level. But on the contrary, in almost every case these spaces were then roofed over at pavement level with short beams, leaving an open passage about two feet high underneath. These passages ran all round the rooms at the foot of the walls, communicating with one another through the doorways. And if it is asked what the purpose of this arrangement was, no obvious explanation is forthcoming, unless one is prepared to accept the possibility that it made possible some form of ventilation in the summer, or even (which seems even less probable), artificial heating in the winter.

As we have said, our proper understanding of the building did not really take shape until the sixth season of excavating in 1959, when we found ourselves dealing with the west wing of the palace. This we found to be considerably better preserved, mainly because, unlike the remainder of the

ruins, it had not immediately been built over by the returning inhabitants of the city after the fire. At one point for instance, near the main entrance on the west side, two pairs of formal niches faced one another across a carefully planned vestibule. Here all the woodwork was preserved in such a perfect state of carbonisation that a "working drawing" could be reconstructed of the complicated structure, including the powerful timber foundations. The rooms behind this vestibule seemed to have been too small for "heating passages" to intrude: but they produced another curious phenomenon. When the ceiling beams had burnt away, the floors of an upper storey had subsided neatly into the room below: and there they lay, superimposed intact upon the ground-floor pavement. These upper rooms must have been comfortable in winter, for their floors were deeply bedded, not in this case with rushes, but with loosely corded bundles of hay or straw.

These fallen floors were of considerable interest architecturally; but they also serve to illustrate the importance in an excavation of exploiting the smallest minor details for the sake of scientific information. The material of which the floor covering was composed, had to be collected with as little disturbance as possible in large polythene bags. It was later sent for examination to Dr. Hans Helbaek of Copenhagen, who is at present one of the most distinguished specialists in the antiquity of domestic plants.[1] Dr. Helbaek's first reaction to the appearance of these specimens showed so much interest and enthusiasm that his own words, in a letter to the excavator, are perhaps worth quoting. "My reaction" he says, "to your straw bedding is one of amazement and delight. Never in my long career have I seen anything so well preserved after millennia of concealment, excavation and long transport. For the first time I now have authentic samples of the cereals which grew 3000 years ago, complete with spikes, awns and the rest of the plant. I spent yesterday and half of last night recovering as much as possible out of the bags immediately, before sifting. In this way I got this unique, completely untouched material—carbonised samples which are usable for direct comparison, without any technical formula calculating carbonisation shrinkage, for comparison with the cultivated crops."

Dr. Helbaek went on to say—"My concept of the bedding is as follows. A thick layer of straw from cultivated cereals with a healthy stem, was laid down once in a while, in the course of time being trodden thoroughly flat

(1) H. Helbaek. *Late Bronze Age and Byzantine Crops at Beycesultan in Anatolia.* "Anatolian Studies" Vol. XI 1961 p. 77.

and probably splintered on the surface during the year. Once a year the top layer was swept out and a cover of fresh straw was added—in this case fetched from the slopes above the site where the wild einkorm abounded. The fire happened not too long after this, as appears from the fact that the fresh straw was still loose when it ignited and got typically carbonised. By comparison the lower layers of bedding were so flat and short of oxygen as to have survived the fire in a 'peaty' state, untouched by the flames." He then concludes from the seasonal condition of the ears most recently laid, that they were reaped in early May, and is thus able to fix the time of year when the fire took place in early summer. This of course corroborates our own theory of the city's destruction, since the conventional campaigning season of the Hittite armies was in the spring.

But Dr. Helbaek in his later and more detailed report of the bedding was able to go further. For, in addition to identifying almost all the crops which were being harvested at Beycesultan in about 1750 B.C., he found, in association with the straw that had already been threshed, a whole assemblage of recognisable weeds, whose time of blooming confirmed the approximate date at which the corn was reaped. So one feels that these deductions made by a specialist, after examining a few pounds of charred substance from the palace, do make an extraordinarily important contribution to the results of the Beycesultan excavations. For now, not only can our own architectural reconstructions conjure up a fairly accurate picture of the actual city and palace, but we are also enabled to imagine very clearly indeed the appearance of the surrounding countryside when the corn was still standing in early May, just before the disastrous fire took place.

We must now return to the more practical details of excavating technique as adopted today in Anatolia. They differ very considerably from those which we have described in Iraq and even more so from Egyptian methods, partly because different implements are traditionally popular. The Anatolian peasant uses a heart-shaped shovel with a handle more than five feet long, and the earth is finally removed in metal wheelbarrows. The long shovel is an extremely efficient instrument; for with it the earth can be thrown a great distance. Throwing from one to another, groups of shovelmen can transfer piles of debris from any part of the excavation to barrows waiting on a prepared track. Similarly, when one is digging a deep trench, earth can be thrown upwards to more than twice a man's height; and

when again groups of shovel-men are disposed on terraces, there is no need for the cumbersome spiral stairway by which, in other countries, basketcarriers manhandle the earth out of a deep sounding.

These Anatolian cultivators have remarkable strength and staying power. We ourselves also found that, with improving educational facilities in the Turkish Republic, many of them took a sober and intelligent interest in the work and needed very little encouragement. If a personal testimony is of interest, in thirty years of experience all over the Near East, one has never seen a higher standard of unskilled labour than one found at Beycesultan. As for the wall-tracing and other more delicate forms of work, we could not, as is now possible in Iraq and elsewhere, draw on a reserve of ready-trained craftsmen. In the early days we borrowed a foreman and one or two wall-tracers from a Turkish expedition. But these men, perhaps naturally, were found unwilling to "spoil their own market" by imparting their knowledge to others: and in the end it was necessary to train a team of local men ourselves. In Anatolia it is once more the long-handled shovel which is used for scraping the surface horizontally in order to find a wall-face; after which a pick and rigid-handled knife are used. Having once mastered this principle, it was no novelty to find oneself again moving from point to point on a dig, correcting the mistakes which untrained men were making, just as one had so often done during the earlier years in Iraq. Soon we had a reliable nucleus of trained men, mostly drawn from the artisan class in neighbouring villages. They continued working for us through successive seasons and were soon prepared to move with us from excavation to excavation in other parts of the country.

Experiences of this sort draw one's attention again to a matter of principle in excavating. One has seen it laid down as an axiom in manuals on field archaeology, that all practical tasks of an excavation should be systematically deputed, and that a field-director should not involve himself in the manual acts of digging.[1] This contention is in such total disaccord with one's own experience and convictions, that it can hardly be taken seriously. To a Near Eastern excavator of any repute it must come as hardly less of a surprise than the photographs which once appeared in a well-known periodical showing how an excavation could be supervised from a glassed-in affair on wheels described as an "observation-car". For what is required of a field-director is that he should be *qualified to conduct an excavation;* that is, that he should be capable of using a properly constituted gang of workmen like a delicate instrument. But he should also ideally be

(1) Sir M. Wheeler. *op.cit.* p. 131ff.

able and willing to take over and improve on the work of any operator who is in temporary difficulties. If, as is in fact often necessary, he must depute the separate sections of his work to assistants, he must himself circulate freely among them or he will lose touch with the sequence of developments on which his archaeological interpretation will ultimately depend. In a word, it can hardly be over-stressed that a field-director is one who conducts an excavation—not someone who afterwards collates the day-to-day notes taken by other people.

There is of course another most testing responsibility which falls to the lot of a field-director. That is the disposal of his available funds to the best possible advantage. There are times when money is short, and the necessity for obtaining "tangible results" from an excavation must be delicately balanced against the ethics of procedure. This again is something which is perhaps best illustrated by instances of situations which have actually arisen, and for this purpose two examples suggest themselves, each connected with one of the earlier archaeological ventures undertaken by the Ankara Institute.

The first of these took place in 1949 and was concerned with a small mound on the outskirts of the market-town called Polatli, about sixty miles west of Ankara.[1] Mounds of this sort, upon which a modern town has encroached, usually suffer from the townsman's need to obtain earth to make bricks. The Polatli mound was no exception and it had been quarried for this purpose until less than two thirds of it remained. In the process some interesting Bronze Age pottery and metal objects had come to light, which eventually found their way to the Ankara Museum. A test excavation seemed desirable before the quarrying went any further. Hearing of this, I found that a sum of £100 could be made available from the Institute's still rather meagre budget, and offered to collaborate with the Turkish Antiquities Department if they could contribute a similar sum. With this very modest financial backing our first field operation in Turkey took shape.

Clearly this was a case where one's first purpose should be to obtain a stratigraphical cross-section of the mound's occupation: but neither the funds nor the time at our disposal would have sufficed for a single large-scale sounding from the summit downwards. The alternative seemed to be a series of small test-trenches, each one stratigraphically connected with the next. And for this purpose the existence of the quarry proved extremely useful. The first two trenches were made in the undisturbed summit of

(1) Reported in "Anatolian Studies" Vol. I. 1951.

the mound. These two, stratigraphically, brought us down to the lip of the quarry. Two more were then made in the vertical cliff created by the quarry itself, where the earth could be conveniently tipped over the edge. And finally a fifth and sixth trench was cut in the horizontal floor of the quarry down to the original virgin soil. In this way, in a matter of only three weeks' digging, the full range of thirty occupation-levels were tested and the pottery from each analysed. Once more the analysis could be presented in diagrammatical form; and this diagram proved extremely revealing. All the principal interruptions in the cultural history of Central Anatolia could be recognised and fixed chronologically; and these have since been correlated with the newly established sub-phases of the Early and Middle Bronze Ages. Not everyone would have regarded this as a conventional operation, since it was undertaken with a staff of three people only and not a single skilled workman was available. But the results suggest that a mere two hundred pounds could hardly have been more usefully spent.

Our second venture of this sort presented a much more complicated economic problem. In 1951 I found myself, still in collaboration with the same Turkish colleague, excavating the huge mound called Sultantepe, a few miles from Harran on the Turco-Syrian frontier.[1] The Institute was by then in the third year of its existence, and this was the first major excavation which I had undertaken on its behalf: so the urge to obtain results was perhaps unusually strong. But our budget, even when increased by a Turkish Government contribution, was once more very modest indeed. Sultantepe is one of those sites which is still occupied by a large modern village: but in this case the village can no longer occupy the summit of the mound, because it is too high. (PL. 3) Standing as it does no less than a hundred and fifty feet above the plain, it is only accessible after a rather stiff climb: and one then looks down on the roofs of the modern village below. I had been partly attracted to the place by the presence, halfway up the slope, of two enormous granite column bases, sprawling at odd angles among the debris, and obviously dating from the late seventh century B.C., when Harran was an important city of Assyria. So we started excavating, not at the summit of the mound but a little above the level from which these column-bases had fallen. In this way, what one might call the chronological anatomy of the mound very rapidly became clear. In Assyrian times it had already stood to a height of over a hundred feet and had a flat top, on which the Assyrians had constructed some sort of

(1) Reported in "Anatolian Studies" Vol. III 1953 in calloboration with Bay nuri gökçe.

religious acropolis with a columned gateway. It seemed likely that it enclosed a temple dedicated to the Moon-God, "Sin of Harran", in which case there was a good chance of our finding tablets and other inscribed material.

All this I discovered by cutting vertical slices off the sloping sides of the summit, down to the Assyrian level, and thus exposing the periphery of the acropolis buildings. It was done throughout with scrupulous attention to the rules. Not only were all the building remains in the post-Assyrian levels planned, but careful sections were recorded as every slice proceeded. And the results, in the first season, were more than encouraging. Our anticipation of a Sin Temple standing in its own acropolis was confirmed by inscribed material. The walls of the acropolis were located and traced, where the erosion of the mound had not removed them, and a peripheral plan of religious buildings inside the walls was made. One of these buildings contained the now-famous Assyrian library, consisting of six hundred religious texts, instalments of which are today, ten years later, still being published.[1]

But at the same time our vertical cuts had revealed a most disconcerting picture of the post-Assyrian levels which constituted the summit of the mound. After the destruction of the acropolis in 610 B.C. the site was reoccupied, and all through the Hellenistic period there was a small village on top of the hill. Then the Romans adopted it as a military outpost and it was intermittently occupied by the barracks of their garrison. The result was an accumulation of debris over the Assyrian remains, between twenty and twenty-five feet deep. When eventually we had laboriously cut a trench across the summit, all this was impressed upon us. There was an interesting collection of Pergamene pottery, both Hellenistic and Roman and there was a little Parthian glazed ware, to show that the Romans had not always had it their own way. But the architectural remains were negligible—the poorest hovels in Hellenistic times, and so little building in Roman times that one gained the impression they had lived "under canvas". The trench cost rather over £200.

So here was an economic predicament of the sort which we have set out to illustrate; a predicament which might at any time face an excavator with limited funds at a site to which access was not easily obtainable. Twenty feet beneath the summit of the mound, as our peripheral excavation had shown, were Assyrian remains of great importance. (One chamber

(1) Instalments in "Anatolian Studies" and *The Sultantepe Tablets*, British Institute of Archaeology at Ankara, Occasional Publications No. 3 by O. R. Gurney.

inside the acropolis had even shown signs of being ornamented with painted frescoes). But to reach these the entire summit of the mound would have to be removed. We calculated that if this were done conscientiously, level-by-level, with the budget at our disposal it would have taken at least three complete season's work and have been likely to prove almost totally unrewarding in objects or information.

Some day some other excavator may find it worth his while to face this disability and complete the excavation of the Sultantepe Sin Temple. I myself was relieved from the quandary by the Institute's decision on the most adequate grounds, to transfer its activities elsewhere.

CHAPTER VI

Finding and Choosing Mounds

One subject to which only passing reference has been made in the foregoing chapters, is the matter of choosing a site for excavation. A good deal must now be said about the principles which should ideally be applied to the process of selection. It may be interesting first to recollect how little attention was paid to these in the Near East until comparatively recent years. In Iraq for instance one need hardly go further than the sites whose excavation has already been mentioned, to notice the wide variety of chance circumstances which led to their discovery.

In this respect, we should of course ignore the greater and more famous ancient cities such as Babylon, Nineveh and Ur, because their remains were so conspicuous and local tradition had retained some memory of their identity. Once the cuneiform script had been deciphered, it needed only a casual examination of the inscribed surface-material to confirm it. But the smaller and less conspicuous sites were discovered in other ways. Just as, in the last century, the French excavator, Botta, was introduced to the site of Khorsabad by local workmen excavating for him at Nineveh, so also in the nineteen-twenties, S. Langdon and his excavating party at Kish were shown a tiny mound in the neighbourhood called Jemdet Nasr, which has since given its name to a prehistoric period. Woolley and Hall could hardly have failed to notice the ruins of the little temple platform at Al'Ubaid, since it was visible on the horizon while they were at work on the summit of the Ur *ziggurat*. But even more common throughout the Near East are the cases where such sites as these have been discovered through the curiosity of peasant cultivators or the activities of illicit diggers. Apart from the innumerable tombs and cemeteries in Egypt which were found in this way and afterwards looted, there is the rather shocking example of the famous Luristan Bronzes, which are now so liberally distributed among the museums and private collections of the world. In the Lurish province of western Persia, from the year 1930 onwards, between four and five hundred burial grounds, each containing approximately two hundred graves have been excavated commercially without the assistance of any archaeologist. This also was the kind of situation which led to our own excavation of the

Diyala sites, east of Baghdad. We have already described how Mr. Sidney Smith, as Director of Antiquities at that time, found the shops of the Baghdad dealers so over-stocked with tablets and Sumerian sculpture from Tell Asmar and Khafaje, that it would have been thought there could be nothing left to find at the sites themselves. But the discovery of the two subsidiary mounds, Tell Agrab and Ischali, were the result of our own explorations, carried out in such spare time as we had during the excavating season. And for our own part, this was our first introduction to a novel proposition; the possibility of systematic archaeological reconnaissance.

The initiative in this matter was not our own. Already in other parts of the Near East, colleagues of several nationalities had understood the possibilities of systematised explorations in which all the mounds and other ancient settlements in a given area could be investigated and recorded. One of the pioneers in this form of experiment was M. E. L. Mallowan, then working for the British School of Archaeology in Baghdad. In 1934 he became interested in the richly cultivated plain which is an extension of North Syria towards the Tigris and which the French then called the Bec-de-Canard. Between the years 1934 and 1938 he made elaborate and thorough surveys, based on the two rivers Khabur and Balik, which spread their tributaries over this area, mapping the position of innumerable ancient settlements and estimating the period of their occupation by the surface-finds.[1] In each case several hundred mounds were recorded and two of them, Tell Brak and Chagar Bazar were selected for excavation by his own expedition. The Chicago Oriental Institute was also at the time contemplating an excavation in that area, and was prepared to pay two hundred and fifty dollars for an advance copy of Mallowan's report. So for the first time this system could be placed on a practical as well as a scientific footing.

When I myself returned to Iraq in 1939 as adviser to the Antiquities Department, I found that my Iraqi colleagues had already begun to understand the value and practical convenience of such operations. At that time the process of Land Settlement, by which the ownership of real estate could be legally determined, was still in progress. Parties of Tapu officers and surveyors were operating for this purpose all over the country, including the most remote parts. It was therefore arranged that each Land Settlement camp should be attended by an Antiquities Inspector, whose duty it was to claim any sites as crown property, to record their geographical position

(1) Mallowan's survey of the Khabur valley was reported in "Iraq" Vol. III 1936 p. 1ff.: his work in the Balikh valley in "Iraq" Vol. VIII 1946 p. 111ff.

and to collect from each a bag of surface pottery for examination in the Iraq Museum. This of course proved an admirably effective arrangement and soon began to produce results. As the bags came in, the information provided by their contents sent us on errands to all parts of the country to examine mounds whose description seemed of particular archaeological interest. We have already described elsewhere how such journeys resulted in the excavation of sites such as Tell 'Uqair and Hassuna. But these were only isolated products of the system. By the time I left Iraq in 1949 more than five thousand mounds had been correctly located on large-scale maps and against each were numbers symbolising the periods at which it was known to have been occupied.

Meanwhile the system had been carried a stage further in neighbouring countries. R. J. Braidwood, who was at that time directing an Oriental Institute expedition in North Syria, had taken the initiative in this respect. The site which he was excavating, Tell Judaidah, was in the 'Amuq Plain east of Antioch;[1] a wide area of farmlands, between the Amanus mountains and the Orontes, which must have been a curiously self-contained province throughout antiquity. In the flank of Tell Judaidah Braidwood cut an enormous step-trench, which, in the course of several seasons' work, gave him an archaeological cross-section of the mound's history back as far as the fourth millennium B.C. But as a supplement to the actual excavation he had also conducted a methodical survey of the whole 'Amuq province, during which he located every ancient settlement in the area. And now, because all the surface pottery picked up on these other mounds could be identified and dated by the position in which it appeared in the Judaidah sounding, something quite new could be accomplished. A series of maps could be produced showing the number and distribution of towns and villages in the 'Amuq Plain at each of the historic and prehistoric periods represented in the Judaidah mound. Prosperous and impoverished periods could in this way be distinguished and the main market-towns or agricultural centres identified. In the years which followed, Braidwood's publication called "Mounds in the Plain of Antioch"[2] became a model for such surveys, and many new ones were undertaken covering large and important areas of the Near East. (PL. 30) In some cases it was possible to connect them with the chronological framework provided by the excavation of a particular mound or mounds. And of this one might take as an example the province of Cilicia in southern Turkey, where the excavations

(1) Cf. R. J. Braidwood. Excavations in the Plain of Antioch. Chicago 1960.
(2) Oriental Institute Publication No. XLVIII Chicago 1937.

at Mersin and Tarsus provided a key to the chronology of the whole area painstakingly surveyed by Miss Seton-Williams.[1]

During our early years in Turkey, a member of the Ankara Institute, James Mellaart, undertook to apply this system on a very large scale to the greater part of western Anatolia. Operating at first alone and later assisted by other members of the Institute, he succeeded in covering such a wide area that, in 1954 the time seemed to have come to take stock of the situation and to consider how the information which he had gained could be used to the greatest advantage, With so much experience of this type of research, we were then able to tabulate some of the principles involved and to come to certain conclusions.[2]

First then, one wished to define the purpose of these surveys. It seemed to us that they should be aimed at obtaining two kinds of evidence, which are themselves complementary and their significance interdependent. The first is topographical and concerns the character and distribution of ancient settlements. It should be obtained by carefully distinguishing different types of mound—villages, market-towns, administrative centres—and those with special purposes such as fortresses, burial-tumuli and so on. The second is material evidence provided by surface finds—particularly by a systematic study of the pottery. If no effective excavation has yet taken place in the area one is dealing with, one's first step is to take the entire assemblage of surface sherds and set about a systematic analysis and classification, using for comparison any available material from neighbouring areas whose date and provenance is satisfactorily established. For the time being any completely new material has to be laid aside with notes regarding the circumstances of its discovery. But very often at this stage it will already be found that useful maps can be made showing the distribution of particular wares and techniques. Also a preliminary idea can be obtained of the historic or prehistoric periods at which each mound was occupied. If all this is done painstakingly and typological diagrams are also concocted to demonstrate the chronological evidence, it will usually be found that inferences can be made under the following headings.

First, the distribution map should begin to show the existence of what one may call "cultural provinces"— areas in which particular ceramic forms were popular at particular periods. Secondly one should be able to gauge the comparative density of population at any one period (as Braidwood was able to do after his survey of the 'Amuq). Next, one should see

(1) Reported in "Anatolian Studies" Vol. IV 1954.
(2) Cf. S. Lloyd. *Mound Surveys* in "Antiquity" No. 112 Dec. 1954 p. 214ff.

pointers to the economy of a particular period, as suggested by the distribution or grouping of settlements; for example agricultural villages around a central market-town, hill-settlements in a mining area, or walled fortresses located to meet a specific threat, like the little Chalcolithic fortress at Mersin which guarded the western approach to the Cilician plain. Then also, one should sometimes be able to see the direction of caravan routes indicated by lines of mounds. We have spoken elsewhere of how these mounds often occur at the point where the road crosses a stream or river. And finally, by comparing one's evidence with the written records, of actual military campaigns, one should obtain new ideas about historical geography and the identification of ancient place-names. For so many arguments on this subject take place between philologists who have never themselves actually visited the country under discussion or understood the geophysical problems involved.

Let us then return and consider a specific example of how the above principles can be applied to the results of such a survey and of the practical benefits which result. In the spring of 1954, the Ankara Institute was planning a large-scale excavation somewhere in western Anatolia; and in choosing a site a clearly defined archaeological problem was involved. There will be no need here to recapitulate the argument in detail, since the excavation itself is now long-since completed and the results fairly well known. It may suffice to say that our primary objective was to locate and investigate any surviving remains of the Bronze Age State called Arzawa, which is so frequently referred to in the national archives of the Hittite kings. It was then, and unfortunately still remains, one of the enigmas of Anatolian history that, whereas the Hittite records leave no doubt as to the size and importance of this western neighbour-state, which they themselves were apparently never able effectively to conquer, virtually nothing is known about its history and only its approximate position on the map was at that time suspected. Our intention then was if possible to define its geographical limits and to excavate one of its larger cities.

But there was to be a second and more general purpose in our excavation. The western half of Anatolia was at that time a sort of archaeological no-man's-land, almost unbelievably little being known, particularly about its prehistory. All the information we had, depended, first, on Blegen's excavations at Troy, which, as we have already suggested, were considered to have closer connections with the Aegean and Greece than with the Anatolian mainland. For links with inner Anatolia we depended almost exclusively on Dr. Winifred Lamb's pioneer excavation of a west Anatolian

market-town at Kusura near Afyon Karahisar. As for the prehistory of the area, one remembers the firm assumption at that time that Anatolia proper for some reason remained unoccupied until very late Chalcolithic times, and the pains with which we delimited the western spread of prehistoric cultures to the thousand-foot contour at the foot of the Taurus mountains.[1] As for the Neolithic, someone had found obsidian implements near the Salt Lake in the centre of the plateau: but these were assumed to have been left by travellers from the east in their search for salt.

It may truthfully be said that, without further excavation, J. Mellaart's West Anatolian survey made nonsense of all these theories.[2] He had found an extension of the Mersin type of Chalcolithic painted pottery reaching far out onto the plateau from the Taurus passes. With the help of a clue provided by Professor K. Bittel, he had also discovered a cluster of most promising-looking Neolithic settlements around the beautiful lake called Beysehir, west of Konya. And he had traced an elaborate pattern of Bronze Age sites from the Taurus to the Aegean. Clearly it was time that some large West Anatolian site should be excavated in depth and so provide a chronological skeleton on which all these cultures could be hung. The secondary aspect of our own problem then, was to find a site where this objective could be attained, in addition to the solution of our historical conundrum regarding Arzawa.

So it was with a strictly practical purpose in view that we tested the efficiency of Mellaart's distribution maps, which already covered the greater part of the area where Arzawa could be expected to have been located. Particularly, the map of the Late Bronze Age settlements, contemporary with the Hittite Empire, gave one food for thought. Here, in the idiom of archaeological exploration, were two well-defined "cultural provinces", clearly outlined by the character of the pottery distributed over their mounds. They were separated from each other by a firm line running approximately north and south through Lake Egridir: and they were not difficult to associate with historical countries whose existence was implied by the Hittite records. That on the right could be identified as the so-called Lower Land; a state which seems rarely to have attained independence from the Hittite Empire. That on the left however (which was incidentally characterised by hitherto unfamiliar types of pottery), could be seen to comply with all the evidence available in the records regarding the situation of the

(1) Discussed by W. Brice and A. Dönmez in *South East Anatolian Pottery:* "Iraq" Vol. XI.

(2) Some of the results of this survey are represented by ten distribution maps in S. Lloyd and J. Mellaart, *Beycesultan I.*

Arzawan provinces. Furthermore, in the upper Meander valley between Çivril and Dinar, one could see a maximum concentration of settlements, in the centre of which was one very large mound, likely to have been at least some sort of provincial centre. This was Beycesultan. Since it also showed signs of having been occupied over a very long period, starting in Chalcolithic times, it appeared to us most likely to serve the two primary purposes of our proposed excavation.

Something has already been said in a previous chapter about our excavations in the Bronze Age levels at Beycesultan. We have described one huge, "half-timber" building; a residential palace over two hundred feet square and administrative buildings covering an area of almost two and a quarter acres, which we took to prove that in fact, during the Middle Bronze Age, the walled city at Beycesultan was a state capital of Arzawa. But what is more relevant for the moment to the aspect of mound excavation which we are considering is the manner in which we contrived to make this mound a stratigraphical key to the whole prehistory of western Anatolia. For, beneath the Middle Bronze Age public buildings we cut a wide sounding through the earlier occupations down to an earliest settlement on the clean earth beneath. (PL. 7) As we descended level by level, one saw how at last the amorphous evidence provided by Mellaart's west Anatolian survey was crystallising into an articulate pattern, based on this new chronological sequence. Not only were we identifying successive occupations of the mound and noting their characteristics, but for each occupation a ready-made distribution map could now be supplied, showing the geographical extent and character of the material culture which corresponded to it. In fact one may claim that the report on this sounding is in a sense unique.[1] For, in addition to an unusually detailed record of the ceramic sequence throughout the third and fourth millennia B.C., ten maps are published, showing the distribution of each class of pottery at any one of the major chronological periods.

But this prehistoric sounding at Beycesultan is also interesting in the present context for the illustrations which it provides of eccentricities in mound formation. Let us first consider the curious shape into which it developed. The city appears to have stood at the crossing of an important highway over the Meander river; or rather over a branch of that river which is now dry. In an air-photograph the ancient river-bed can clearly be seen winding around the foot of the mound on the south-east side. So the hill itself, which stands seventy-five feet high, has two separate summits

(1) S. Lloyd and J. Mellaart *op.cit.*

and a saddle between, over which a cart-track still passes. Our sounding at first took the form of a fifteen-foot trench cut into the eastern flank of the west summit, facing towards the central depression. As it grew deeper, an eastward extension was made in the form of a very narrow passage through which the earth could be extracted in wheelbarrows.

At the end of the 1956 season we had reached an Early Bronze Age level about contemporary with the second settlement at Troy, in the last centuries of the third millennium B.C. Here we were beginning to recover a lot of very distinctive pottery of the type which one usually describes as "votive vessels". At that moment we were deepening the wheelbarrow trench, and discovered suddenly that it was cutting across something which must at all costs be carefully recorded; a building, evidently a shrine, containing an altar-structure which consisted of two upright stelae and a pair of clay "horns" such as one finds in Minoan architecture of a much later date. Only a few hours remained before the end of our season's work: so the trench was temporarily closed. In the following spring, a greatly enlarged shaft was marked out, so that the whole buiding could be properly investigated. This initial discovery then subsequently led to the clearance and recording of five sanctuary-chambers, which were the first religious buildings of the Early Bronze Age to be discovered in Anatolia. One of them has been the subject of a pictorial reconstruction, showing all the votive pottery in place as it was originally found. And one sees how the offerings were evidently passed by the worshipper over the "horns" and between the stelae to a waiting priest on the other side.

But an interesting circumstance about these shrinerooms was that they were evidently built on the outside of the settlement with their ends abutting against an outer enclosure-wall. And as we descended into the Chalcolithic levels beneath, again and again this same enclosure-wall appeared, cutting across our sounding, with buildings inside but only a "fall-out" of accumulated rubbish on the outside. This continued almost down to the original settlement at plain level, and it presented us with a very curious problem. For the wall was facing towards the saddle-shaped depression which cut the mound into two halves. It might have been possible to suppose that in the earliest times, here on the west side of the highway was a walled city, and that an extension to it on the east side was not added until the Bronze Age. But this was not borne out by the facts. For our investigations in the eastern hill showed that an approximately equal number of occupations were represented beneath it.

One was finally compelled to imagine a settlement which had started in

Chalcolithic times with a few houses on either side of the road near the river-crossing. In the course of time this grew into a much larger settlement, eight-shaped, with the road still passing through the centre. And when the time came to fortify the place, each separate half was enclosed in its own protecting wall. With this rather curious explanation we have had to be contented.

The sounding reached clean soil eighty feet beneath the summit of the mound; five feet, that is, beneath the old river-bed and ten feet at least beneath the surrounding cultivated plain. This was of course explained by the fact that, over a period of six thousand years, the continual flooding of the river and accumulation of top-soil, had raised the habitation-level in the valley to that extent. But it also impressed on one how many of these small Chalcolithic villages, if for any reason they ceased to be occupied at an early stage, could be completely covered up by rising alluvium, leaving no trace on the surface. And this, at the time our excavation ended, was a depressing thought. Because our work was by no means over. The earliest occupation at Beycesultan corresponded to the beginning of what has come to be called the Middle Chalcolithic period. But there was still an Early Chalcolithic and even a long Neolithic occupation of Western Anatolia to be investigated. Should we find that villages dating from this period were mostly abandoned and covered by alluvial soil? Or would they be inaccessible beneath high accumulations of later occupation? A somewhat reassuring answer to these questions was already provided by Mellaart's maps, which showed, as we have already mentioned, promising groups of early sites in situations unaffected by flooding rivers or rising alluvium. At some of them also, these very early remains appeared to be conveniently close to the surface.

So the next phase in this programme of Anatolian research is represented by Mellaart's own excavations at one of these sites.[1] This was Hacilar, not far from Burdur in the region which we have come to call the Anatolian "Lake District". It certainly deserves some mention here, if only because it provided yet another excellent example of the unpredictable stratigraphical eccentricities which one is likely to encounter, even in a small and uncomplicated mound formation.

Hacilar was not one of the mounds recorded in Mellaart's survey. It stood no more than twelve to fifteen feet high and might well have escaped the notice of anyone whose attention was concentrated on more conspic-

(1) Preliminary reports on the Hacilar excavations appeared in "Anatolian Studies" Vols. VIII (1958) to XIX (1961).

uous mounds. So its discovery was due, first to chance finds made by a peasant proprietor while ploughing, and afterwards to illicit digging by an enterprising villager. This brought eventually to the hands of Istanbul dealers examples of brilliantly painted and attractively designed Chalcolithic pottery, recognisably earlier in date than any previously found in western Turkey. The excavations began on a small scale in 1957 over an area at the summit of the mound, and the source of the painted pottery was immediately identified as a tiny prehistoric village of mud brick, surrounded by a roughly rectangular enclosure-wall of the same material. The houses were very small and irregularly built, but they had elaborate domestic arrangements; hearths, dome-shaped ovens, wooden doors (sometimes sliding back into the walls) and screened courtyards with kilns for baking pottery. Leaving these houses exposed, Mellaart extended his excavations to a lower point on the flank of the mound, where thicker walls seemed to portend a much larger version of the same village. And sure enough, here, outside the area of the walled village and at a considerably lower level, he found houses with walls as much as five feet thick. He noted that they were locked together, side by side, as though to form a fortification around the perimeter of the settlement, which was itself on a larger and more ambitious scale than the one he had already cleared. (PL. 28)

But now symptoms began to appear of a disconcerting stratigraphical paradox. The perimeter buildings of the lower settlement did not seem to extend inwards beneath those of the smaller ones. Furthermore, archaeological evidence began to suggest that the former was later in date than the latter. The problem was of course solved by cutting a broad connecting trench between the two, and making a close study of the section. In this one could see with remarkable clarity an almost vertical earth-face connecting the two levels, just inside the lower peripheral buildings; and one could infer that the ruins of the smaller settlement must already have been in existence when the building of the larger settlement was planned. Before it was built, a wide terrace had been cut around the full circuit of the mound. Upon this the peripheral buildings had been constructed to form a roughly circular fortification, and the remains of the older settlement had been levelled to make a central enclosure, perhaps for cattle or for a refugee population in time of danger. Only an experienced excavator could have arrived directly at this conclusion without a long period of preliminary confusion. The evidence produced by a mere "sounding-in-depth" would certainly have proved totally misleading.

The larger (and as we now know, later,) settlement at Hacilar had other

very interesting features. Judging from the plan which Mellaart recorded, the actual dwellings, with their heavy walls and absence of doors or windows, appear extremely uncomfortable. But they had been destroyed by fire and the burnt debris inside them produced an unexpected revelation. It showed that there had been an upper storey constructed almost entirely of timber (and also possibly corbelled out beyond the outer faces of the walls to give greater floor-space, like the upper rooms in a Turkish village house). It was evidently here that the main functions of family life had taken place. For all the domestic pottery and other paraphernalia had fallen with the burning beams into the brick chambers below when the upper floor collapsed. With them Mellaart found gruesome indications that some of the inhabitants themselves had not escaped.

Mellaart continued these excavations every summer until 1960. By then he had penetrated clean through the Chalcolithic remains and as expected he had reached a late Neolithic village beneath. Since, in a sounding of this sort, one is moving progressively backward in time, there is an illogical tendency to expect progressively more primitive and elementary conditions of human life. It was in this case therefore a little surprising to find that the reverse was the case. The Late Neolithic village was composed of large rectangular houses, extremely well built. Again the material was mud brick, without timber reinforcement in the walls. But their flat roofs, which had to span distances of up to fifteen feet, were supported on wooden posts or pillars, usually arranged in pairs. Each house had a main living-room showing a standard arrangement of planning, with a central doorway in the long side. Directly opposite the doorway on the opposite side, was a domestic hearth built up in clay to a rectangular shape and a rather elaborate baking-oven, sometimes set in a formal recess in the back wall. In other cases two such recesses were arranged symmetrically on either side of the oven. The roof supports were also placed symmetrically.

In any other context these details might appear to be of trivial importance. But in the deeper levels at Hacilar we are dealing with the early years of the fifth millennium B.C. This means that, with few and equivocal exceptions, we have here the earliest examples of consciously formal planning in the history of architecture. But, in other ways too, these Neolithic people were culturally far in advance of the Chalcolithic settlers who replaced them at Hacilar. It was for instance in these strongly built and well arranged living rooms that Mellaart discovered the extraordinary collection of fifty or so baked clay figurines, which created so much interest among art historians when they were published in 1961; women, clothed

or naked, sometimes with children or tame animals, seated, lying or sprawling in strangely unconventionalised attitudes. One has been identified as a goddess seated on a leopard throne.[1]

Beneath these houses, and before reaching virgin soil, Mellaart found traces of an even earlier occupation of the site. But here at last he appeared to have reached a really primitive form of domestic life, in which, as one might have anticipated from excavations at Jericho and elsewhere, even pottery was not yet made, nor apparently were houses built of a durable material. Only, again as at Jericho, one could conclude that some sort of ancestor worship was practised; for human skulls were found, propped up with pebbles in carefully chosen positions. But in any case he did not consider his work to be finished. For between his ancestor-worshippers and his figurines there was an evident hiatus in the occupation of the part of the site which he had excavated; a chronological interlude perhaps amounting to several hundred years. This gap had somehow to be filled.

So once more there was a sequel to the Hacilar excavations at a site called Çatal Hüyük in the Konya Plain, which Mellaart had noted in his survey as the evident remains of a huge Early Neolithic settlement.[2] The excavation at Çatal Hüyük is still in its early stages; but to judge by the preliminary reports, nothing could have confirmed with such astonishing emphasis our anticipation of a precociously advanced Neolithic civilization in western Anatolia. What Mellaart has discoverd is not a village nor even a large settlement, but indisputably a town of considerable pretentions. By comparison for instance with Troy, it can reasonably be described as a city, since an area of almost twelve acres is covered with large and formally planned mud brick houses. Inside these houses, in addition to fragments of sculpture in-the-round and in relief, artistically far in advance of their time, Mellaart found walls covered with painted frescoes, sometimes applied to plaster faces modelled in relief. Since the scenes depicted, though more naturalistically drawn, are similar to those in Palaeolithic cave-paintings—hunting scenes and ritual celebrations performed by people wearing the skins of animals—one feels that here at last is one long-awaited link in the connection between the cave-dwellers of the food-collecting age and the Neolithic food producers. There seems little doubt that the further excavation of this site may produce a spectacle in relation to which Jarmo and Jericho will seem like mere curtain-raisers.

(1) Cf. photographs and line drawings in "Anatolian Studies" Vol. XI 1961.

(2) First announcements of excavations at Çatal Hüyük appeared in 'Archaelogy', Vol 15 No. 1 and 'Anatolian Studies', Vol XII, 1952.

Our original intention in this chapter was to discuss the principles governing the choice of a site for excavation. To illustrate the application of these principles, I have dwelt at considerable length on our eight-year programme of archaeological operations in western Anatolia, because it seems to me so effectively to have complied with all the requirements of the situation. First came the preliminary surface exploration, co-ordinated in this case into a systematic survey covering the whole region under consideration. Information obtained by this means served on the one hand to illuminate its division into cultural provinces, thus enabling us provisionally to identify the semi-historical province in which we were interested. On the other, it brought into focus a sequence of cultural periods and facilitated the selection of a site where their comparative chronology could simultaneously be investigated. When the stratigraphical sequence at this site proved incomplete, it was found that it could be supplemented by the investigation of two further mounds, one of which again owed its discovery to the original survey: the other illustrated the alternative possibility under such circumstances of profiting from the conclusions to be drawn from chance finds by peasants. So here for our consideration is one type of approach to the formulation and execution of an archaeological programme with a predetermined scientific objective.

But there are other and contrasting situations, which differ from the above in that the reasons for selecting a site are irrelevant to the problems which it is likely to present. There are for instance "rescue-operations" like those we have described at Khafaje and Tell Agrab or, to quote a more extreme case, like those at present in progress behind the new High Dam in Lower Nubia. And there are also of course great cities like Ur, Babylon and Nineveh, whose historical fame has engendered interest in their remains. In the early days of archaeology, such considerations as these were often the preponderant motives behind an excavating enterprise. It was not in fact until comparatively recent times that archaeologists seem to have become aware of the ethical principles which governed such matters. And here once more, full credit for the development of a conscience should be given to a generation of specialists in the field of Roman Britain, and above all perhaps to the late Professor R. G. Collingwood, that great pioneer of rational archaeology.

It is interesting to remember that, as late as the nineteen-thirties, Collingwood was still able to find fault with the orientation of British field archaeology. He says in his autobiography that at that time—"excavation was still being done according to the principles laid down by General Pitt-

Rivers in the last quarter of the nineteenth century. Pitt-Rivers was a very great archaeologist and a supreme master of the technique of excavation. But as regards the problems to be solved by archaeology, he was for the most part in the pre-Baconian stage. He dug in order to see what he could find. He had not applied to archaeology the famous advice of Lord Acton, 'Study problems, not periods'. Among his successors, as I found, archaeology meant studying not problems but sites. The idea of archaeology was to choose a site: to uncover it systematically, one piece each year, pouring thousands of pounds into the work, until it was all dug; and then to go on to another. The result was that, although museums were choked with finds, amazingly little (as it now appears), was discovered about the history of the site."

Certainly there would seem a great deal to be said for Collingwood's contention that an excavation should be directed towards a particular problem and that, what he elsewhere describes as "blind digging" should if possible be discouraged. But let us now see to what extent this stricture can be applied to Near Eastern archaeology at the present time. And let us for the purpose take a specific instance of an excavation to which, at first and superficially, it would appear to be applicable. The site called Nimrud in northern Iraq was first excavated by Layard in the middle of the last century, and there is no doubt that he excavated there "to see what he could find." The excavations were resumed by the British School of Archaeology in Baghdad in 1949 because the director, Professor M. E. L. Mallowan, felt that the School had a moral obligation in regard to the site. Its original exploration had been a British enterprise and many of Layard's finds had gone to the British Museum. But the excavation had been carried out under difficult conditions: primitive methods had been used and it had never been properly completed. For these reasons the site was chosen. The problems to be solved presented themselves as the excavations proceeded. One problem was in regard to the exact chronology of the city's occupation: and in order to refute at once any suggestion that here—"nothing was discovered about the history of the site," let us record one specimen contribution of this sort, made during the most recent excavating seasons.[1]

Among the tablets found at Nimrud, business transactions are recorded through several successive reigns and are particularly plentiful in that of the last king, Sin-shar-iskun. Of these latter three in particular could be dated reliably to the year 615 B.C.; the latest year of the Assyrian era ever mentioned in Nimrud inscriptions. In themselves they were of no great significance; but, thanks to a long and patient examination of the strati-

(1) Information kindly supplied by Professor M. E. L. Mallowan.

graphic circumstances in which they were found, made by Mr. D. Oates who was latterly in charge of the excavations, they contributed to the determination of an exact date for the ultimate fall of the city. Mr. Oates reached his conclusion in the following manner.

It was known from the Babylonian Chronicle that Nineveh fell to the combined forces of the Medes and Babylonians in the year 612 B.C. and that the King of Assyria thereupon fled north-westward to Harran, where a brief and unsuccessful attempt was made to rehabilitate his court. In 610 B.C. the Assyrian monarchy and government ceased to exist. It is accordingly certain that from 612 B.C. onwards Calah (Nimrud) could no longer have been in Assyrian hands. Now, the evidence obtained while excavating the building at Nimrud called Fort Shalmaneser, showed that the first destruction of the city, a violent conflagration, was followed very shortly afterwards by a partial reoccupation. The fact that this was an orderly reoccupation under Assyrian authority could be inferred from the simultaneous installation in conventional positions of "foundation-boxes" containing magical figurines, and also from the absence of any change in the ceramic typography. Some attempt had moreover been made to salvage valuable objects from the ruins caused by the fire and the mutilated ivory furniture had been carefully stacked. Here then were signs of a deliberate attempt by the Assyrian government to re-establish order in the city. And this must clearly have taken place before the sack of Nineveh—perhaps in the previous year, 613 B.C., when it is known from the Chronicle that Assyrian fortunes temporarily recovered. This then would suggest that the original holocaust at Calah might have been in 614 B.C. The absence of business documents dated later than the previous year would point to this and indeed, we know that the same year saw the destruction of two other famous Assyrian cities, Assur and Tarbisu. A picture is thus created of two stages in the final conquest of Assyria, with a respite in between during which a temporary illusion of security returned to the citizens of Calah.

This is but one of many historical details recovered at Nimrud. Twelve successive seasons of excavating, during which the written records were scrupulously related to the stratigraphical evidence, have thus added the final paragraphs to a chapter of Assyrian history whose recovery was initiated by Layard just over a hundred years ago. Even Collingwood would perhaps have hesitated to complain that museums were consequently "choked" with Nimrud ivories.

In these last paragraphs we have touched, albeit briefly, on the more important considerations ordinarily affecting the choice of a site for excavation. Some excavators have been seen operating over wide geographical areas in their search for evidence which might contribute to the solution of specific problems. Others we have watched effectively probing such problems at sites whose selection had been governed by other and irrelevant considerations. And finally we have noticed how excavations were sometimes re-opened at famous sites whose potentialities could not be considered to have been fully exploited. May we not now carry the examination a stage further? Having dealt with the choice of a site, should we not now turn our attention to the qualifications of potential excavators? If we do so, we shall find ourselves dealing with the one outstanding aspect of Eastern archaeology which remains to be discussed. For we have still to decide what form of instruction should ideally be given to students, who think of themselves as potential field archaeologists.

And here, since it is only right that one should accept full responsibility for personal convictions which may at some point in one's career have unconsciously deteriorated into prejudices, let me revert for a moment to the use of the personal pronoun. I myself, then, am convinced of, if not obsessed with, the necessity for some form of *field apprenticeship* to form the background of such training. This may seem a simple expedient to advocate; but the difficulties which all instruction of this sort presents are equally obvious. A good field-director, in the process of conducting an excavation, is usually too greatly and exactingly preoccupied to overload himself with heavy teaching responsibilities. Also he has his budget to consider. On excavations in the Near East, the remoteness of the site often creates practical difficulties in catering and transporting equipment, which greatly increase if a class of students is added to the complement of qualified assistants. Only the most generously endowed expedition can in these circumstances avoid over-taxing its resources. And this probably applies exclusively to those which contribute to the activities of a teaching institution. In such cases it is clearly right that learners should participate in considerable numbers: and a formal routine has long ago been devised for keeping them profitably occupied, by allotting to each nominal responsibility for supervising a small part of the excavation. But it should be said at once that this does not correspond to my own definition of the term "apprenticeship". However firmly the written rules of archaeological procedure have been instilled into one, it is not enough to be installed in an isolated "trench" with half a dozen unskilled workmen and left to one's

own devices pending the director's periodical visit. My own contention is that those privileged to learn should as it were be taken into the director's confidence. They should circulate with him or with the assistant who is conducting each major part of the excavation, so that they may hear and take part in the perpetual discussion (and indeed arguments), through which the meaning of archaeological evidence is analysed and logical inferences are decided upon. It is only in this way that a learner may eventually come to understand what one may call the tactics or detective-work of an excavation and, for that matter, the economic handling of the very large labour gangs that are used in the Near East.

There is no intention in all this to belittle or subvert the principles at present applied to archaeological training, for instance in the United Kingdom. These have now raised British Archaeology to the status of an exemplary discipline. But in the Near East it is perhaps the aims of an excavator which are different. He does not require and often cannot cater for a large body of junior assistants with academic qualifications in ordinary field practice; for much of what they would do is already done by locally trained staff. What he does need is, in the first place specialists; trained surveyors, draughtsmen, photographers, ceramic and conservation experts. Secondly he will wish for one or two privileged trainees, in whose ability his confidence is sufficient to justify personal attention. To these he will continue to impart all the practical instruction of which he is capable, until they themselves are qualified to direct some independent project. Perhaps then, the two classes of training between which we are attempting to distinguish —disciplinary training at home and apprenticeship abroad—should rather be thought of as separate and successive phases in a prolonged course of instruction. Mound excavators of the future may then regard the one as an essential preliminary to the other.

And as the future is momentarily in mind, let us finally consider the prospect with which they will now be faced in the world of Near Eastern research and to what goal their activities can now most profitably be directed. If they hope for problems to be solved, there will be no lack of them. There are still so many and such leading questions to which we should like to know the answers. And in tackling these problems, one hopes that they may sometimes be actuated by the impulse to "break new ground." When excavators of a younger generation have appeared in recent years, there has often been a tendency amongst them to prefer re-opening old sites, already partially excavated, which have so far in the past failed to supply the required information. Their motives in doing so are under-

standable and largely commendable. They are symptoms of a healthy distaste for a job left unfinished, and also the conviction that, by applying improved methods and exercising greater patience, the desired results may be ultimately forthcoming. But there is a sort of academic timidity about this policy which seems in practice to have inhibited the pace of their progress towards really illuminating discoveries. Some few have already taken the other, more independent course and finds like those at Hacilar and Çatal Hüyük have resulted. Others might now emulate this policy, by looking for new sites with specific lines of enquiry in mind. Examples of the latter are not difficult to suggest. In Egypt the stratigraphic content of mounds in the Delta is a subject which has until now been regrettably neglected. In Mesopotamia the civilization which produced our "Ninevite Five" pottery still remains an enigma. We should like to find and explore a Sumerian city on the Upper Tigris. We should also like to locate and excavate Agade, the capital of the Akkadian Empire, or Washukkani, the capital of Mitanni: and we should like to know whether the lost city of Opis lies beneath the ruins of the Hellenistic capital at Seleucia. And finally—a question to which there must somewhere be a logical and simple answer—there is the problem of Mesopotamian royal burials. In Egypt, as is well known, the name of practically every Pharaoh in the dynastic lists is by now associated with a particular tomb or tombs of known provenance. In Mesopotamia the situation makes a curious contrast. In the city of Ur, Woolley found and excavated the tombs of certain Third Dynasty kings, which were rifled and empty. But, though the names of more than a hundred rulers appear in the Mesopotamian king-list. These and certain plundered tombs at Ashuv are the only historical burials yet located. No systematic search ever seems to have been made for a formula to explain where and how the remaining rulers were buried.

But even if we restrict ourselves to more modest forms of archaeological aspiration, so much remains to be learnt. As A. E. Housman once said—"The sum of things to be known is inexhaustible and however long we read, we shall never come to the end of our story."

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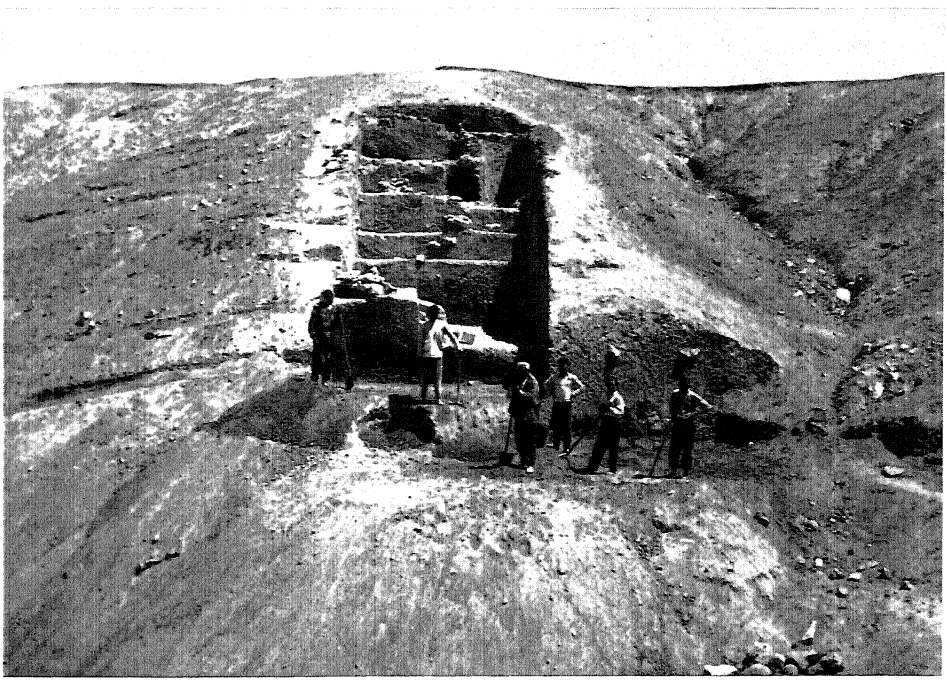
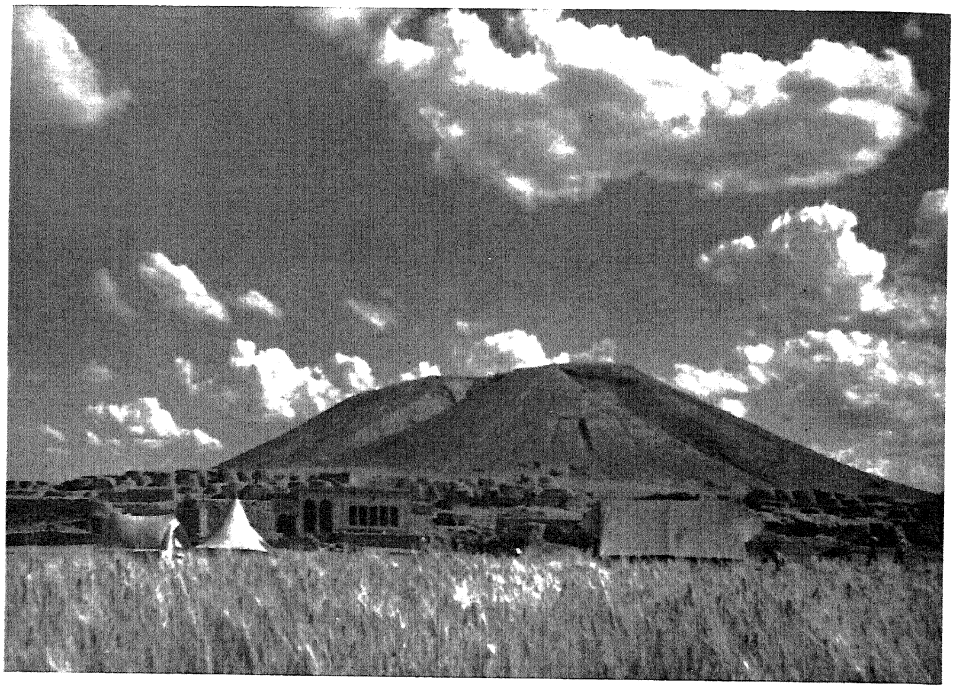
Map of the near east showing principal sites discussed in the book

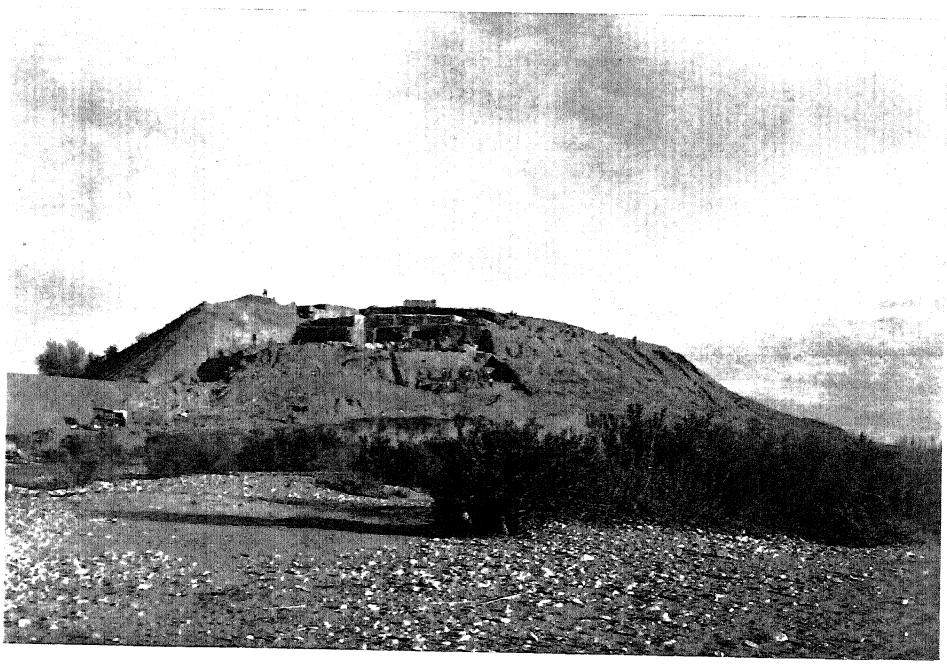
1. Erbil: Old city on mound.



2. Stepped sounding at Sultantepe.

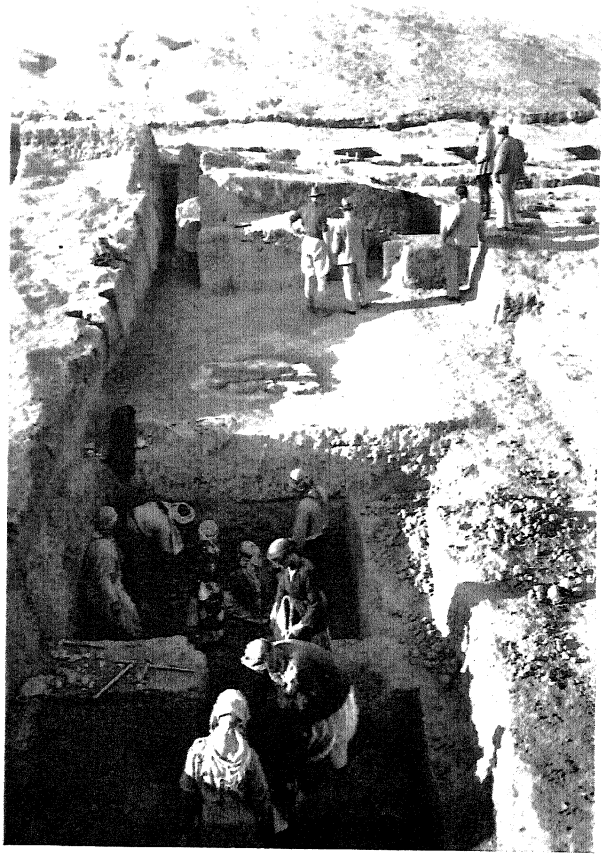


3. Sultantepe: Mound and village.

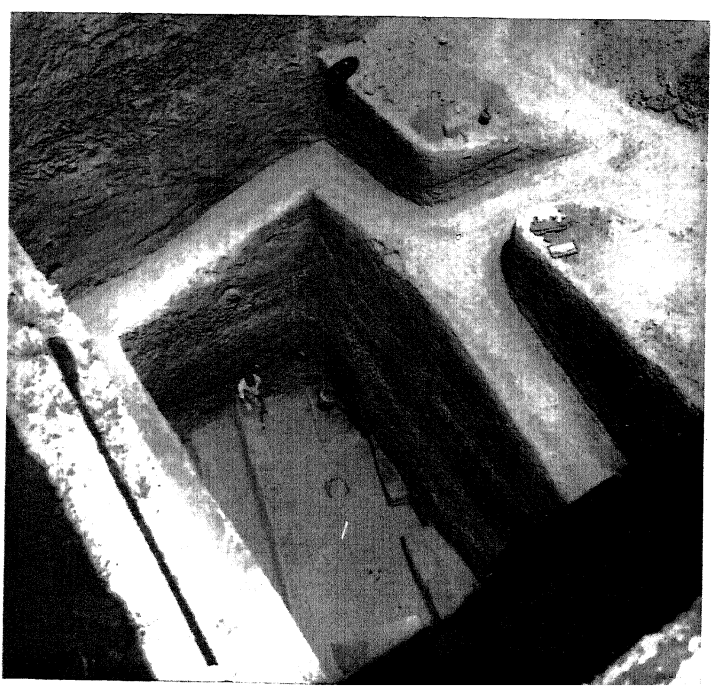


4. Mersin: Excavations in Yümük Tepe.

5. 'Aqar Quf: Characteristic mud brick excavation.

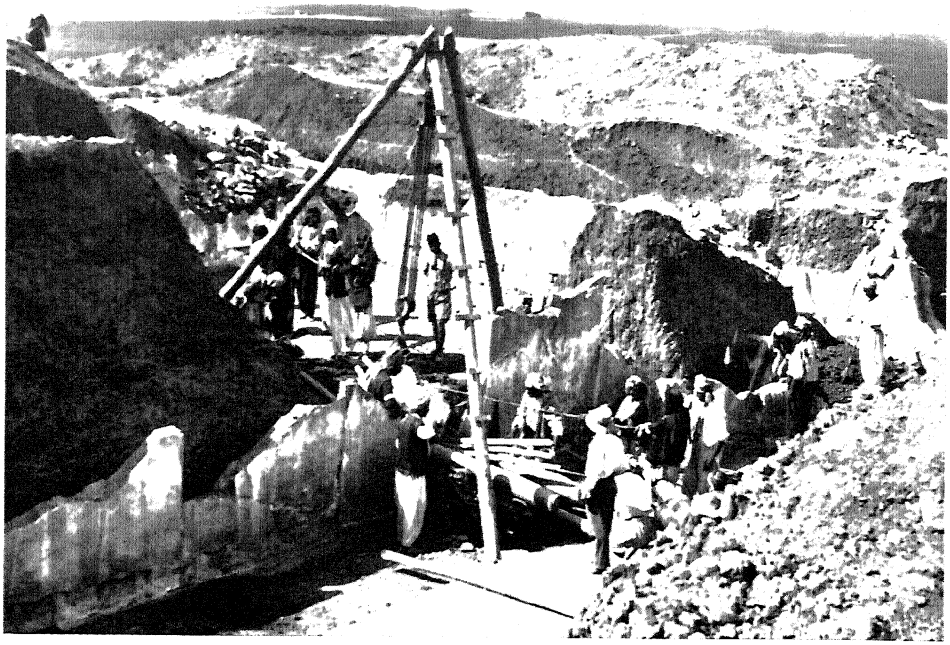


6. Tell Agrab: Sanctuary of Shara temple.

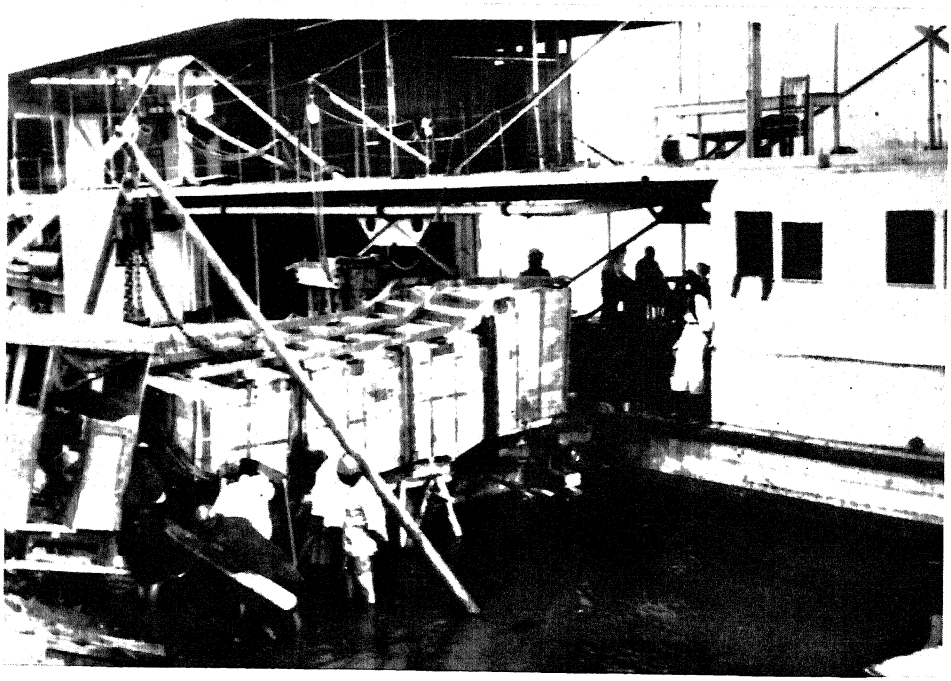


7. Beycesultan: Deep Sounding

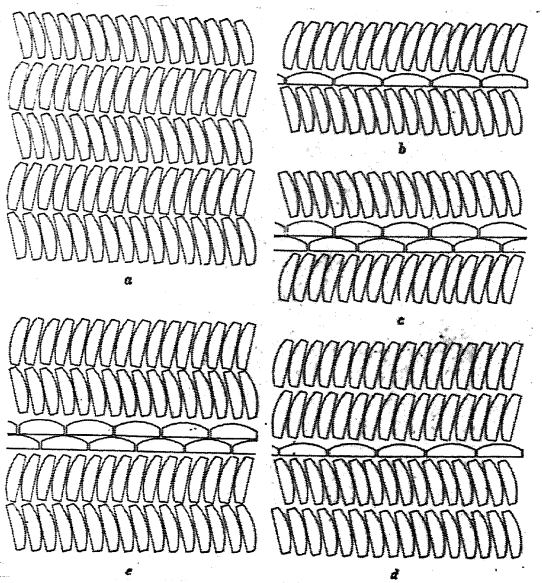
8. Excavations at Tell-el-Amarnah.



9. Khorsabad: Recovering damaged reliefs.

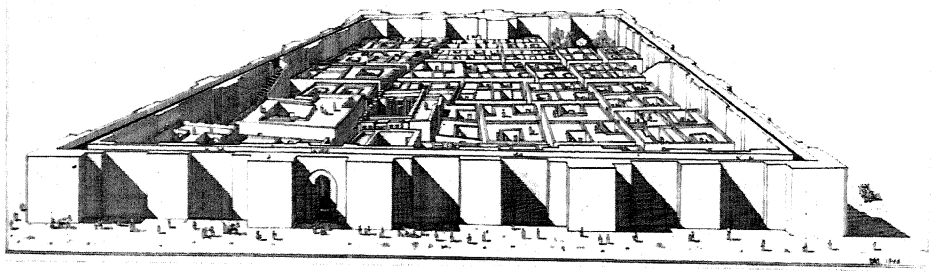


10. Khorsabad: Primitive transport for sculptures.

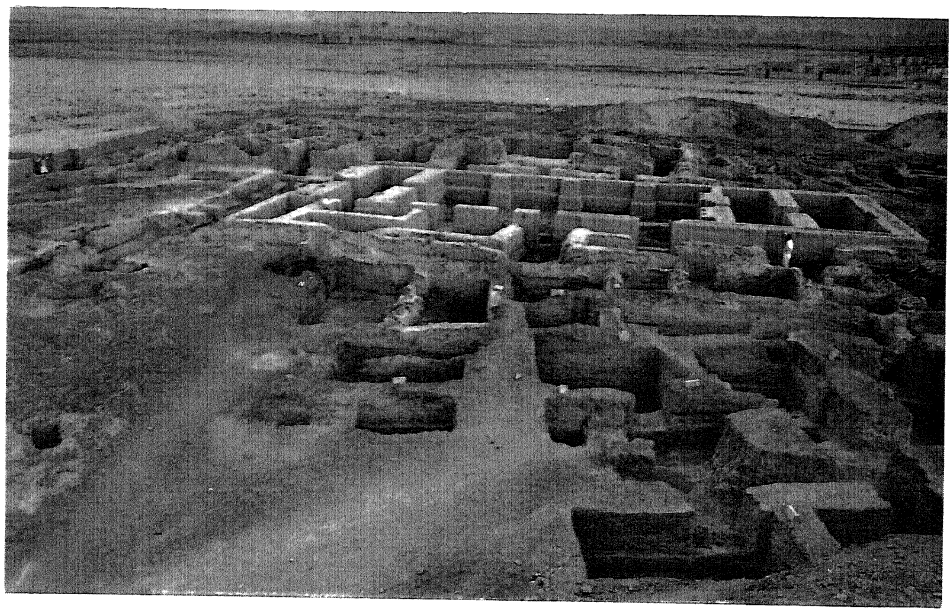


11. Khafaje: Bonding of plano-convex bricks.

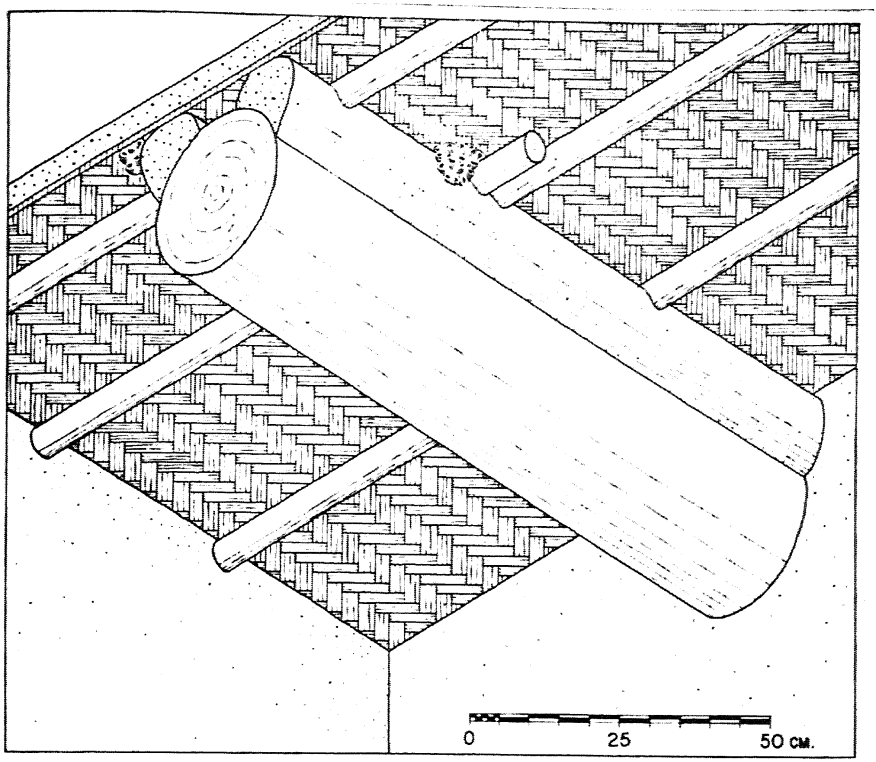
12. Khafaje: The Oval temple.



13. Tell Harmal: Walled town reconstructed.

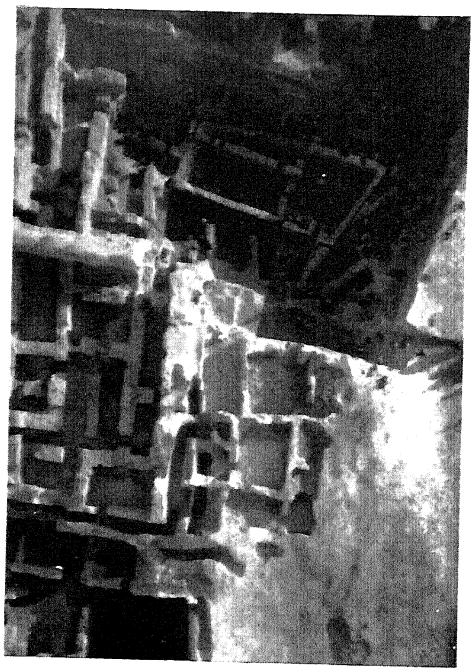


14. Tell Harmal: Excavation with temple walls restored.

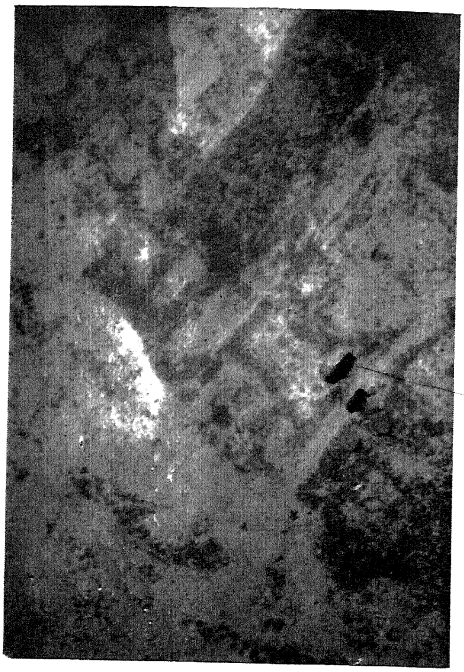


15. Khafaje: Beam construction and wasp's nests.

16. Khafaje: The Sin temple.



17. Tell Asmar: Kite photograph of Abu Temple.

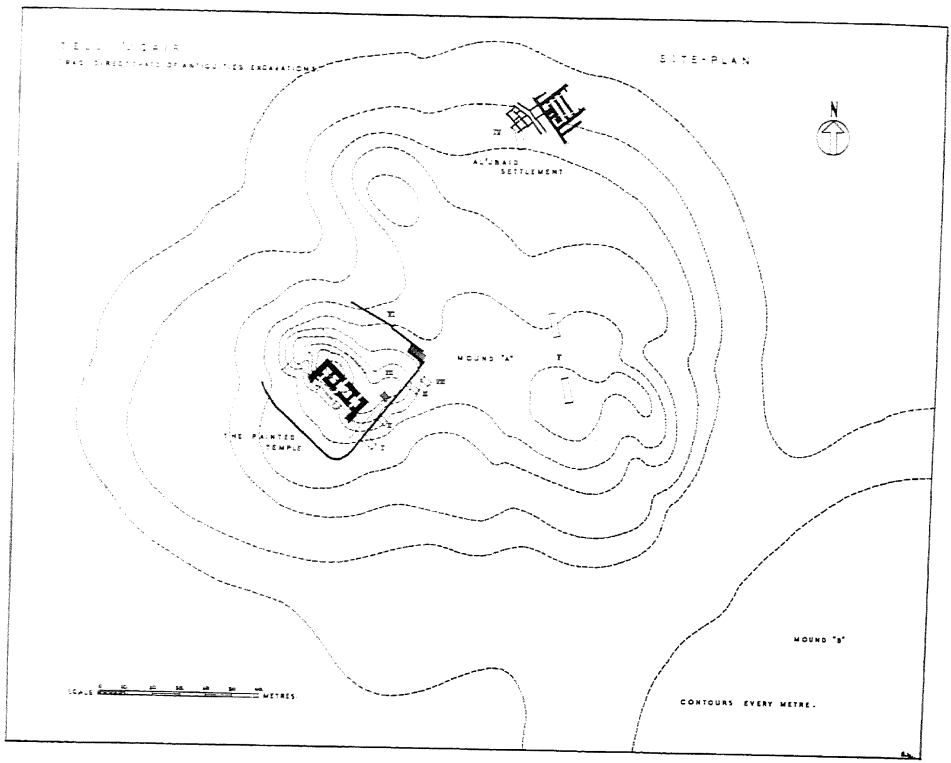


18. Tell Asmar: Kite photograph showing surface after rain.

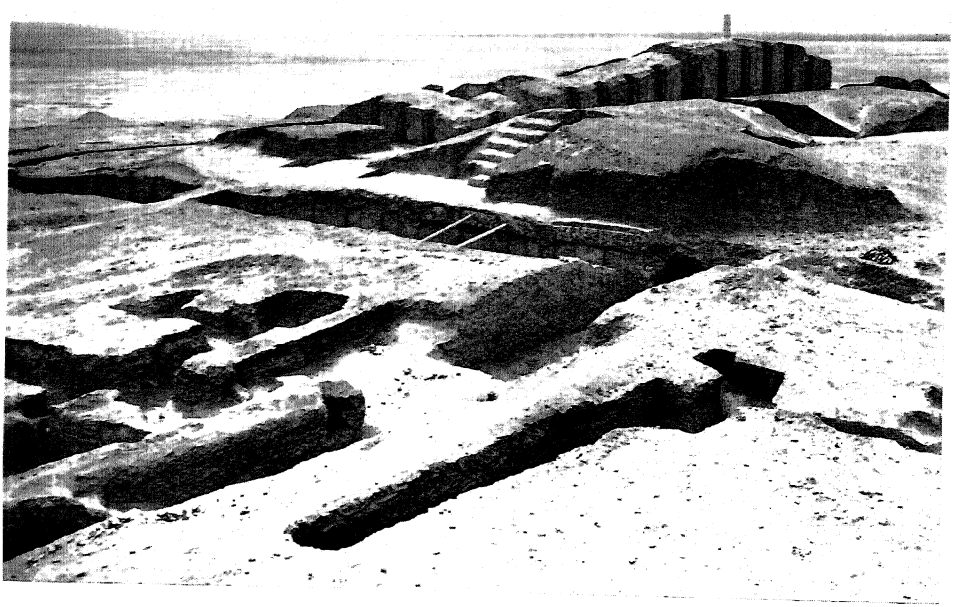
19. Tell Asmar: Excavations from the air.

20. Tell Agrab: Shara temple after excavation.

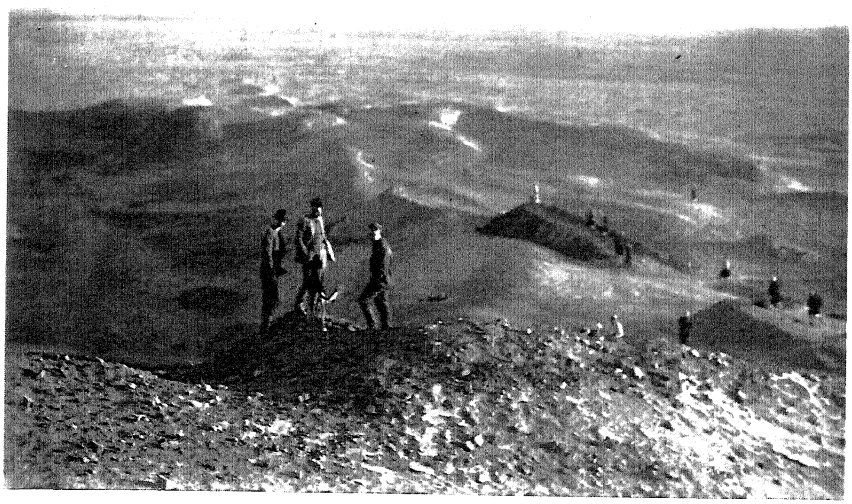
21. End of the Iraq Expedition.



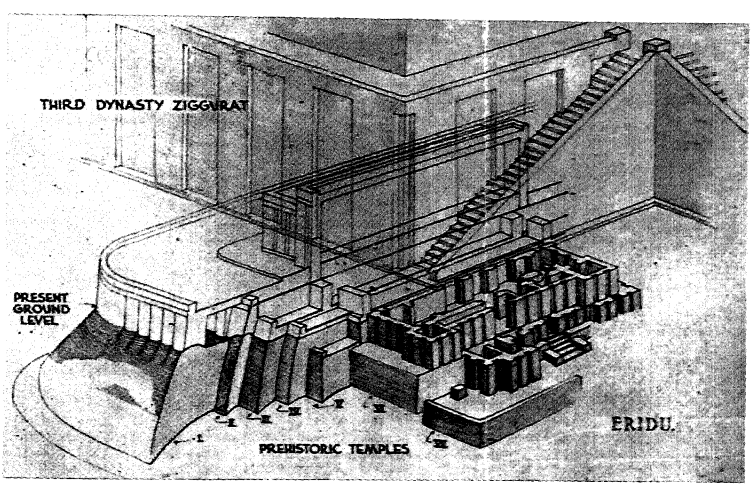
22. Tell 'Uqair: Site plan.



23. Tell 'Uqair: Temple after excavation.

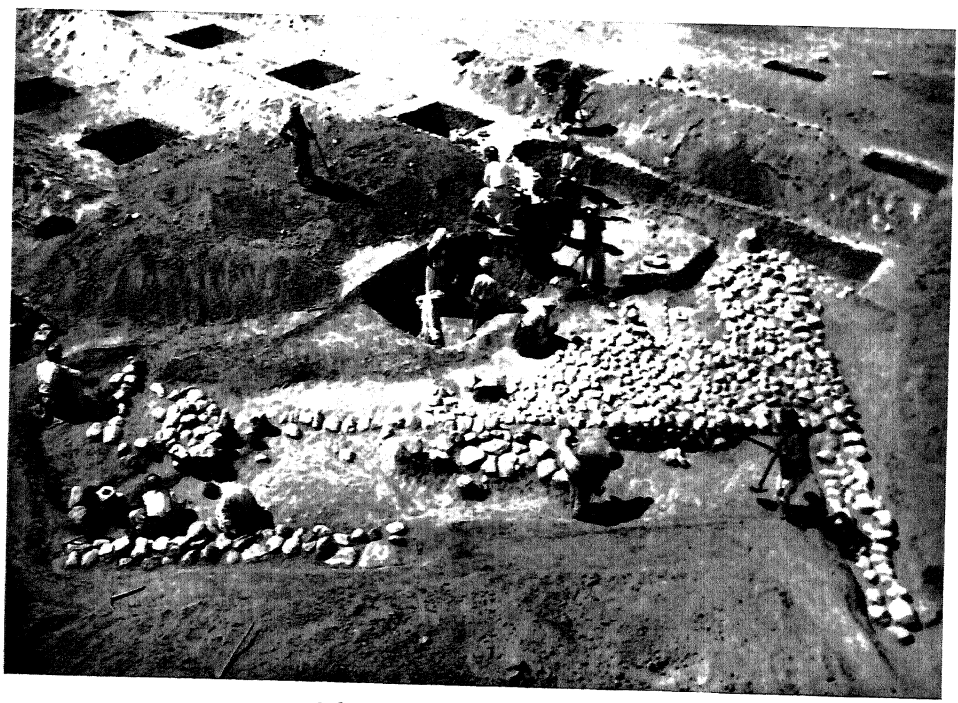


24. Eridu: From the summit of the ziggurat.

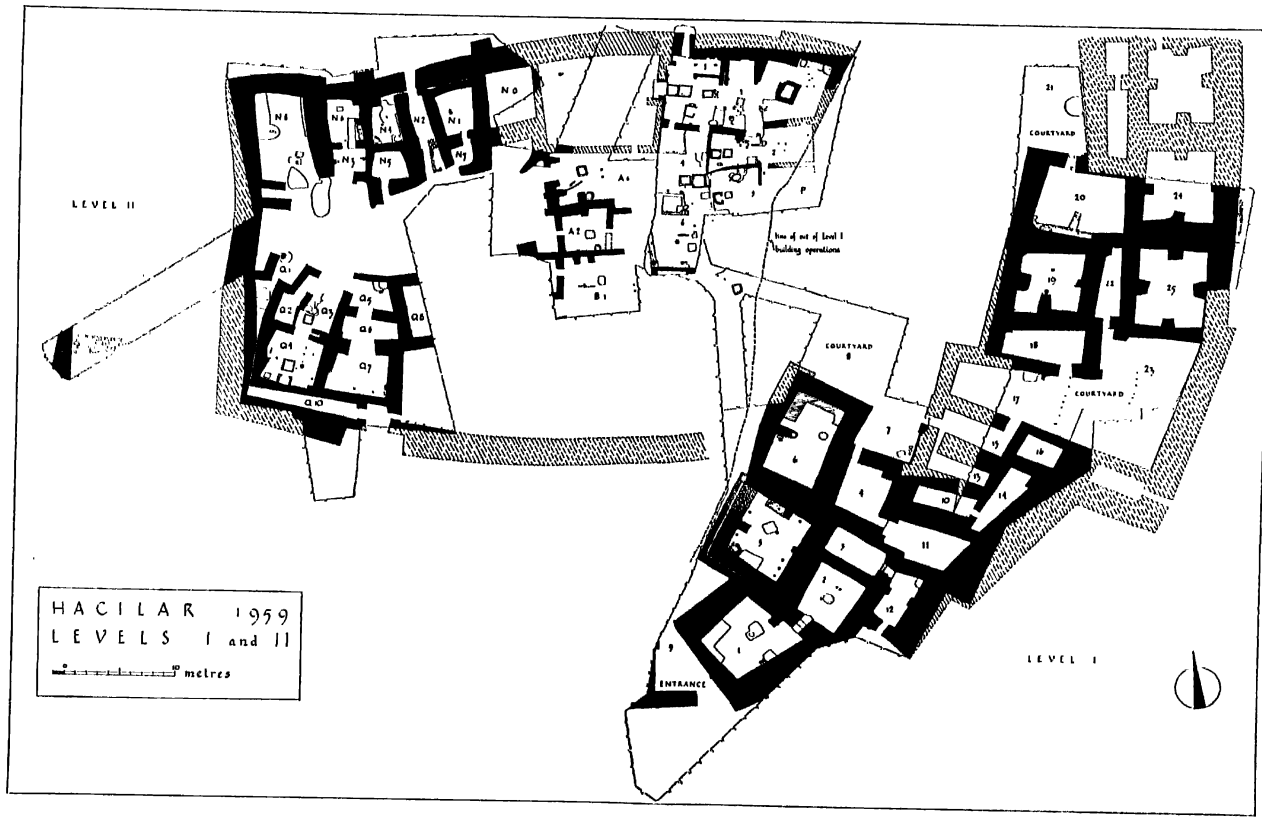


25. Eridu: Diagram of temples beneath the ziggurat.

26. Qal'at Jarmo: Summit excavations.

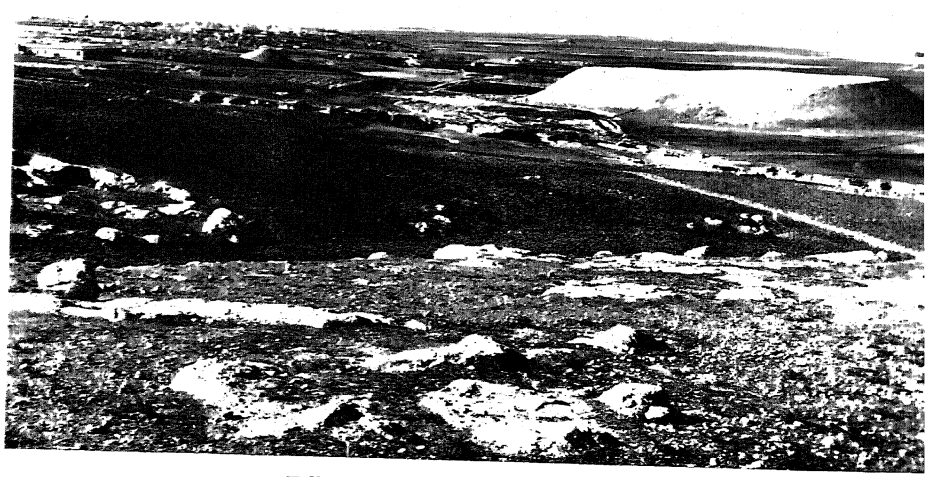


27. Qal'at Jarmo: Excavation and test-pits.



28. Hacilar: Plan of Levels I & II.

29. Beycesultan: Ruins of Burnt Palace.



30. Tell Judaidah: Mounds in the Plain of Antioch.

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